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THE NEW HARVARD ENTRANCE REQUIREMENTS[1]

In this address (a free abstract of which, written out later, is here given) it should be clearly borne in mind that the speaker is familiar only with eastern admission arrangements, and that what he says is wholly from the point of view of an eastern institution, with its own problems, in some respects different from those which meet the schools and universities of the west. Indeed, a knowledge of the real entrance requirements in any institution, as distinguished from the catalogue rules, can be gained only from an acquaintance with actual practise, for in the nature of the case the real requirements depend on the mode of administering the rules.

I

The earlier entrance requirements at Harvard, as at all contemporary institutions, were determined by the fact that the subjects studied in school were generally to be continued in college. It was accordingly necessary to know whether a boy who applied for admission had reached the point in Latin, Greek and mathematics where he would be able to go on with college work in those subjects. This was substantially the case until soon after the beginning of the administration of President Eliot in 1869. A process of radical modification in the Harvard entrance requirements then began, and at successive periods since, about ten years apart, there have been important and far-reaching changes.

[1] Address at the annual meeting of the Michigan Schoolmasters' Association, Ann Arbor, Mich., March 31, 1911.

The chief points in these changes appear to have been three: (1) a substitute for Greek has been provided; (2) with the development of the elective system the subjects studied by college students have in most cases become different from those which they have pursued in their school courses, and it is consequently necessary to learn not the degree of their attainment in Latin, Greek and mathematics, but whether they are competent to carry on studies in history, economics, modern languages and science; (3) it has been intended to aid the schools by setting in the several examinations a standard for school work to which the schools can hold up their boys. In pursuance of this last idea there has been a tendency to provide examinations in some subjects in which hardly any boys were likely to offer themselves, but which some schools wished to teach, and in which the college was told that such an examination standard would be found valuable.

In the successive changes made at Harvard, chiefly in 1871, 1878, 1886 and 1898, can be seen the working of these various motives, and especially can be traced a gradual process by which the substitute admitted for Greek, at first partial, has become complete, and has finally been made not much, if at all, more difficult than the Greek requirement.

The present plan of Harvard entrance requirements was adopted by the faculty in 1898, although the form of statement has since become somewhat changed. It includes a large number of optional subjects, many of them having a weight of not more than one "point" in the system. These options are in some (and the most important) cases real, in the sense of being practically available for schools, but in most cases they are illusory, because very few of the schools from which boys come are equipped to fit boys in these less usual subjects. The chief technical peculiarity of the Harvard system is that the numerical values attached to the different subjects are not based wholly on the relative time supposed to have been expended on those subjects in the high-school course, but have been adjusted on the theory that work done in the last two years of the high-school course ought to be given a higher rating than the work of younger boys. Accordingly, in determining the "ratings" a coefficient was introduced corresponding to the stage in the school course at which the subject would commonly be studied.

II

Under this system of complete examinations for all high-school studies, which differs but little in theory from that of the other eastern institutions where examinations are required, some good results have been felt in the schools from the establishment in certain fields of study of definite standards tested by a college examination; and in general the system has provided a method, though an imperfect one, of selecting from the whole body of applicants those who were best fitted to undertake college work. About 75 per cent. of those applying have usually been admitted to the freshman class, as is shown in the table given below. At the same time certain bad results have been more and more clearly perceived, both from the point of view of the college and from the side of the schools. The latter, indeed, have not been slow to present complaints. And these bad results seem to be necessarily consequent upon the system itself. The gradual perfecting of an inherently defective type of machine has naturally brought out more and more clearly the working of its defects.

1. The system has resulted in loading a

large part of the freshman class, usually amounting to one half or more, with entrance conditions, and thereby making more difficult the task of the weaker members of every entering class. From the college point of view this difficulty has become intolerable, since it prevents the establishment of a proper pace of work for freshmen.

2. The Harvard system of examinations can ordinarily be prepared for without serious difficulty by any school which devotes itself mainly or largely to that end, and which boys attend for three or four years before entering college and with the purpose of fitting themselves for Harvard. It is not, however, adjusted to the courses of many excellent schools throughout the country—schools wholly occupied with substantial academic subjects and doing first-rate work in those subjects, and it is likely to exclude any boy who makes up his mind late in his school course that he wishes to go to Harvard. In other words, it is a system which was natural so long as resort to Harvard was wholly from private and endowed schools and a half dozen public high schools which made a business of fitting for Harvard and other eastern institutions. If Harvard is to offer the melting pot of a common academic life to boys from many parts of the country, it must adapt itself to the best systems of public education maintained in those widely distant regions. By dictating the whole school course as it does, the present Harvard entrance system unduly restricts the possibility of resort to Harvard College from other schools than that very small number which have for one of their primary objects to be Harvard fitting schools.

3. As a method of selecting the best from the whole body of applicants for admission to the freshman class, the present system is imperfect. It admits to the class a certain number of boys who can do nothing well, but have been crammed to pass every one of the examinations with the lowest pass mark. These boys often get in clear of conditions, but usually come to grief in the first semester. A satisfactory system would exclude them from admission. On the other hand, some have to be rejected who would do well in college if they once got in.

Harvard College does not crave any considerable increase in numbers. What it does desire is the resort of about the same number of young men, but of better students from a wider range of territory. We should like, not a larger number of freshmen, but a larger number of applicants from whom we could make our selection of the best.

As the three evil results already mentioned are observed from the side of the college, so the two following have been urged, and it is believed with justice, by the schools.

4. A system of examinations which, like the present one, aims to test every subject studied in a four-year school course makes it necessary for every subject to be continued in the course in some form until the time for the examinations. Under the system of the German gymnasium the subjects are practically all carried down to the close of the last year, the stream of the minor subjects being kept slender yet sufficient to maintain the flow. The American school system is of a different character, and consequently it is necessary for the American school to review in the last year, or the last two years, those subjects upon which the boy is presently to be examined. This produces a spirit of "cram," deeply regretted by the school-masters, together with a serious overcrowding of the last year, or two years, of the school course.

An elaborate examination of school programs from good schools in different parts of the country recently made at Harvard has fully convinced us that the complaints of the schoolmasters in this matter are justified. If it be urged that the examinations can now be spread over three years, it is to be observed that the college examinations are necessarily adapted to the stage of maturity of boys nearly ready to enter college, and are, consequently, for the most part out of the range of a boy completing the second year of his high-school course.

5. Under the present system of entrance to Harvard College, not only is the course of study in the schools fixed from above, but also the methods of teaching have been dictated by the college. This has taken from the schools freedom to experiment with their own methods of education, a freedom which able and enterprising teachers crave, and ought to have. A certain relief, it must be said, has been found here in the examinations of the College Examination Board, but it appears to be only a partial one.

6. One further bad result upon the schools more directly under the influence of Harvard should be mentioned. The examination system has enfeebled their power to take responsibility for the quality of their own product. As Harvard has undertaken to test every subject studied in the school course, the school has been responsible for meeting these tests, not for maintaining its own ideal and type of education. Indeed, it was hardly open to it to form its own educational ideal or specific type at all.

III

These various incidental bad results of the present system have led the Harvard faculty to a complete reconsideration of the principles which ought to govern a plan of entrance requirements, and to the adoption of a new system which, for the present, will be maintained side by side with the old system, the applicant having his choice whether he will come up for admission under one or the other plan.

In framing the new system the consideration chiefly in mind has been that entrance requirements must always test two things:

1. Whether the applicant has had an adequate school course. Inasmuch as a bachelor's degree represents the completion of the whole course of liberal education, it necesarily includes a guarantee that the earlier as well as the later part of the student's education has been adequate in range and intensity. The college is responsible not merely for college work, but also for knowing whether the school work has been devoted to such subjects as, in its opinion, may properly form a part of the education finally attested by the bachelor's degree.

2. What result has been accomplished by such a course of school study in developing effective ability in the individual boy or girl. This latter test has for its object to determine whether the applicant is likely to be able to do college work well.

Now these two ends for which college entrance requirements exist are entirely different in nature. The character of the applicant's course of study is a very different thing from the practical result in the boy as he stands. The former can be adequately ascertained by proper inquiry and by inspection of the school course he has actually pursued; the latter can only be determined by recitations, examinations, or some similar test, conducted either by the school or the college. The present examination system undertakes to reach both these ends by one instrument—a system of

examinations. The results have been outlined. It would seem better to adopt for each of these ends a method directly contrived to accomplish that particular purpose, and not to try to perform two distinct processes by a machine mainly adapted to one only.

Both in regard to the course of study and to the result of that study, the school and the college have each a distinct responsibility. The general type of school course which is to be accepted as a part of the whole education to be attested by the degree of A.B. or S.B., may, and indeed must, be determined by the college which gives the degree. But under that general type the details of curriculum and of methods of instruction are more likely to be effectively arranged by the school itself than by the authorities of the college. To deprive the school of its freedom and consequent responsibility is to weaken its power of maintaining and pursuing an educational ideal. On the other hand, the testing of the result of the school education belongs to the college, and an adequate test ought to give evidence of general intellectual power, not merely of the faithfulness with which a boy has studied individual subjects at school. The idea that an education consists in absolving individual courses, whether at school or college, is, at the present day, the root of much evil.

In New England, as elsewhere, one of the difficulties of which the schools have complained has been alleviated by the certificate system, which, through the cooperation of the colleges in the New England Certificating Board is now well organized with strict standards. Colleges which thus give to certain schools the privilege of certifying to the preparedness of their graduates for college thereby relax their control, not indeed over the subjects studied, but over the method of instruction in the individual subjects; and this has proved a considerable relief. But the certificate system, at least as organized in New England, violates both of the principles which have been laid down above. Under it the domination of the college over the topics which are to make up the school curriculum, over the relative weight which shall be given them, and over other details which properly belong to the judgment of the school, is quite as close and harassing as under the examination system. On the other hand, that portion of the task which is the rightful prerogative of the college, namely, the determination of how well the schools have done their work, is abandoned by the college and handed over to the headmasters of the schools, who certify, not merely, that the boy has done such and such work in his school course, but that, in the opinion of the master, he is fit to enter college. In both these matters the certificate system has reversed the proper procedure, and puts the responsibility on the wrong side.

IV

Under the influence of considerations like these, the new plan already spoken of has been adopted at Harvard College. The following statement of it has been sent out widely.

NEW REQUIREMENTS FOR ADMISSION TO HARVARD COLLEGE

To be admitted to Harvard College, a candidate

(1) Must present evidence of an approved school course satisfactorily completed; and

(2) Must show in four examinations as explained below that his scholarship is of a satisfactory quality:

SCHOOL RECORD

A candidate must present to the committee on admission evidence of his secondary school work in the form of an official detailed statement showing

(*a*) The subjects studied by him and the ground covered.

(*b*) The amount of time devoted to each.

(*c*) The quality of his work in each subject.

To be approved, this statement must show

(*a*) That the candidate's secondary school course has extended over four years.

(*b*) That his course has been concerned chiefly with languages, science, mathematics and history, no one of which has been omitted.

(*c*) That two of the studies of his school program have been pursued beyond their elementary stages, *i. e.*, to the stage required by the present advanced examinations of Harvard College or the equivalent examinations of the College Entrance Examination Board.

THE EXAMINATIONS

If the official detailed statement presented by the candidate shows that he has satisfactorily completed an approved secondary school course, he may present himself for examinations in four subjects as follows:

(*a*) English.

(*b*) Latin, *or*, for candidates for the degree of S.B., French or German.

(*c*) Mathematics, or physics, or chemistry.

(*d*) Any subject (not already selected under (*b*) or (*c*)) from the following list:

Greek	History	Physics
French	Mathematics	Chemistry
German		

These four examinations must be taken at one time, either in June or in September.

In announcing this plan, the committee on admission wish to point out that it differs in essential principles from the old plan now in use, and that therefore comparisons between the new requirements and the old will be misleading if any attempt is made to express the new requirements in the terms of the old. Under this new plan the college does not intend to prescribe in detail the school course of the boy who wishes to enter, either directly by naming and defining subjects, or indirectly by an elaborate system of rating the studies of a school course in points or units. On the contrary, the college accepts the judgment of a school as to a candidate's program, subject only to the general limitations stated above. It is not necessary, therefore, for a school to fit a candidate's course to detailed definitions of subjects. A good student who has had a rationally planned course in a good school should have no difficulty in proving his fitness for admission, even though his decision to come to Harvard be made late in his last school year. Under the new plan every school maintaining the kind of course indicated will be free to work out its own system of education in its own way. The college, on its part, undertakes only to test the intellectual efficiency of the boy at the time of his graduation from school. For this reason the examinations can not be divided.

A second important difference between the new requirements and the old is the emphasis put in the college examinations upon quality of work. The new plan contemplates examinations different from those now used with respect both to their character and the method in which they will be administered. It is hoped to secure a type of examination which shall be adapted to various methods of teaching, and which shall contain questions sufficient in number and character to permit each student to reveal the full amount and quality of his attainment. In administering examinations under this plan, the committee will always consider examinations in connection with school records, and will endeavor to see not whether a candidate has done a certain prescribed amount of work in a certain way, but whether the general quality of the candidate's scholarship is satisfactory. If a candidate is admitted, he will be admitted without conditions; if he is refused admission, no credit will be given for examinations in the separate subjects in which he may show proficiency, and the refusal will mean that his school record and his college tests do not show that he has the scholarship which makes his admission to Harvard College desirable.

The admission of a candidate under this plan, therefore, depends upon good scholarship as shown in two ways—in his school work and in his college tests. He can not secure admission by scoring points or by working up examinations one or two at a time. He must have done good work in his school according to the testimony of his teachers; and he must meet successfully college tests at the time when he is ready to enter.

In introducing this plan, which departs considerably from schemes of admission now in general use, the college is already aware of various grave difficulties. It will doubtless be difficult to prepare a type of examination paper sufficiently flexible to fit various methods of instruction in various parts of the country, and to enable all candidates to exhibit the full amount and quality of their attainments. To accomplish this end, the committee on admission are authorized to advise

with school teachers in regard to the preparation of papers and the methods and standards of marking; and they confidently hope for the cooperation of schools in working out a plan which they believe will serve the common interests of both schools and colleges.

The scheme, as above outlined, aims to determine by inquiry whether the boy's school work shall be counted as a sufficient preliminary education, and then to test by a sufficient number of examinations, not however, covering the whole school course, what has been the result of the education in the boy's power to do intellectual work. Each of these methods seems apt to the end desired, and careful provision has been made for keeping these two inquiries distinct.

The essence of the scheme is, in fact, that the admission of boys to college is now entrusted to a committee which is expected to use a large discretion under the limits laid down in the regulations. This committee will assemble a sufficient general knowledge of the schools from which boys come, such knowledge as can now be obtained by various trustworthy methods, even from distant parts of the country. It is further provided, through the certified record of the boy presented by the master of the school and through the results of the examinations, that adequate information on the two points emphasized above will be at the committee's disposal.

The restriction upon the type of school which will be allowed to send boys up to become candidates for the bachelor's degree is here made, not, as at present, through the list of examination subjects with their accurate ratings, but through the statement of the course actually pursued by each boy, with the grades attained. The college does not intend to alter at all its policy of requiring that the boy's education shall have consisted mainly of substantial academic subjects. No school course will be accepted which includes any large dilution of manual and technical work.

The examinations are to be of a somewhat different type from those hitherto used, or, at any rate, the treatment of the examinations by the readers and by the committee is intended to be different from that which has been given to examinations in the past. The purpose of the examinations is not to test the work of the several courses of the school, but to sample the boy, as a cargo of cotton might be sampled from taking tests from different representative bales. Further, the object of the examination is not to see whether the boy can get a pass-mark in any one, or in all, of the subjects. It will rather be to bring out how much the boy knows. It is hoped that for his free (fourth) subject he will choose the field he can do best in, and so will be given a chance to exhibit himself at his best. Likewise, it is hoped that the schools will now be able to carry boys to more advanced work in those subjects (as, for example, classics or mathematics) in which they are best equipped—for they can do so with the confidence that the result of that special proficiency in certain subjects will be manifest in the examination, and recognized by the admission committee. The marking of the books under the new system will require that in every case a statement by the reader in words shall give his opinion of the actual quality of the boy as exhibited in that examination. There will be no mechanical adding of grades. It will be impossible to enter on a bare pass-mark in the several subjects. Indeed, it is difficult to say under the new system what would constitute "passing" any one examination. The only "passing" that is contemplated is the evidence, *drawn from the four examinations taken together,* that the boy has at-

tained a satisfactory quality of mind. He is expected to make a *creditable* exhibition of himself, and it is hoped that the system will exclude the boy who, under the old system, probably by the aid of a skilful tutor, can just scrape through every examination. It is hoped that in good schools the new system will make it easier for the school-master to prepare boys for Harvard; but it is not intended to make entrance to college in any way easier for the *boys*.

Inasmuch as the inquiry relates to the intellectual power of the boy at the moment when he stands ready for entrance, it is obvious that the examination can not be divided into preliminary and final. For the same reason, the evil of conditions will be wholly eliminated by the new system. A boy either is or is not fit to enter college. If he is not fit, then he must either abandon the idea or else go back to school and study until he becomes ready. There can be no conditions.

It is not the intention by the new system either to raise or to lower the "standard" of admission. That is to say, it is hoped that in a four years' course the amount of intellectual effort which a boy has to put out in order to prepare for Harvard will be as before. But a smaller proportion of it will be mere cram.

	1906	1907	1908	1909	1910
Admitted to freshman class	576	594	529	573	565
Rejected, or withdrew before completing the examination	166	164	147	197	221
Admitted provisionally as special students	66	40	12	0	0
Total number of applicants for admission to freshman class	808	798	688	770	786
Per cent. admitted to freshman class	71.3	74.4	76.9	74.4	71.9

The present policy of Harvard in admitting students can be seen from the above statistics, which are complete for the years covered.

Of those admitted in the past five years there are undoubtedly some who would have been rejected if they had been compelled to come up under the new system. Many of these have failed, or will fail, to complete their college course, at least with credit. Of those registered, some could probably have gained admission if the new system had been open to them; and of these a large proportion would very likely have shown distinction in their subsequent college course. What the effect will be upon the size of the classes admitted to Harvard College, when most of the applicants shall come up under the new system, is not easy to forecast. There has been some apprehension that the percentage of boys admitted will be considerably reduced, but it is hoped that the larger number of schools which will now find themselves able to prepare boys for Harvard will counterbalance this tendency, and prevent any large reduction in the numbers of the future entering classes.

V

In conclusion, a few words may be added as to the general results which it is hoped to secure from this new plan when it goes into full operation. These results are of widely varying kinds.

1. Harvard College hopes to secure a better body of freshmen, for they will have been selected from the whole number of applicants by more nicely adapted methods, and they will be free from conditions and therefore able to do better work.

2. A large number of excellent schools of the accepted type, now unable in their regular curriculum to fit boys to enter Harvard College, will, it is hoped, under

the new system find themselves able to do so without extra work. This applies to some public schools in New England and to a large number in other parts of the country. At present, there are only fourteen public high schools which have sent to Harvard College one boy a year for the past ten years, and all of these are in eastern Massachusetts.

These two results are primarily significant for the college. The other desired results, if they come about, are broader in their educational significance.

3. Schools of the approved type will, so far as Harvard College is concerned in the matter, gain the freedom which they require for doing their best work, since the new system will make it possible for them to concentrate their efforts by treating more thoroughly fewer subjects, or fewer topics of a subject. The great need of students in schools, as well as in colleges, is that they should acquire a habit of doing well what they undertake to do; the greatest evil in education at present is that students are satisfied with mediocrity.

4. The new system gives some help toward an adjustment of the problem of educating together in one school students preparing for college and students preparing for other callings. It does not wholly solve this problem, but it ought to tend somewhat to relieve it. The problem itself is insoluble. Preparation for a definite vocation must be determined by the character and needs of that particular vocation, and college is a vocation for a young man of seventeen to twenty-one just as much as service in a banking house or factory, and, like those vocations, it has its own conditions of fitness. Different needs can not all be provided for under one system of education. Nevertheless, some parts of a school course are an excellent preparation both for college and for an immediate practical career, and the new system of examinations, under which requirements in specific subjects are kept as high as before but the subjects less closely defined, will, it is hoped, give as much freedom here as the nature of the case permits.

5. The new plan leads away from emphasis on single courses, and insists on the significance of the education taken as a whole. In accord with this underlying idea it is free from all attempts to determine the relative value of subjects as expressed in numerical ratings. In this respect it has a general educational importance, and ought to remove many causes of friction now existing between schools and colleges.

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THE BOLYAI PRIZE. II

INTEGRAL EQUATIONS

In these latter years, Hilbert has above all occupied himself with perfecting the theory of integral equations. We know that the foundations of this theory were laid some years ago by Fredholm; since then the fecundity of his method and the facility with which it may be applied to all the problems of mathematical physics approve themselves each day with more luster. This is certainly one of the most remarkable discoveries ever made in mathematics, and for itself alone it would merit the very highest recompense; if to-day, however, it is not to the first inventor, but to the author of important improvements, that we have decided to award the Bolyai prize, it is because we must take into consideration not only Hilbert's works on integral equations, but the totality of his achievement, which is of importance for the most diverse branches of mathematical science and of which the other parts of this report permit us to appreciate the interest.

But we can not enter upon this subject without paying homage to the immense service which Fredholm has rendered to science.

The theory of Fredholm is a generalization of the elementary properties of linear equations and determinants. This generalization may be followed up in two different ways: on the one hand, by considering a *discrete* infinity of variables connected by an infinity of linear equations, which leads to determinants of infinite order; on the other hand, by considering an unknown function $\phi(x)$ (that is to say in last analysis a *continuous* infinity of unknowns) and seeking to determine it by the aid of equations where this function figures in integrals under the sign $\int$. Upon this second way Fredholm has embarked.

Let $K(x, y)$ be a function we call the *kernel;* the integral

$$\psi(x) = \int K(x, y)\phi(y)dy,$$

taken between fixed limits, may be regarded as a transform of $\phi(x)$ by a sort of linear transformation and be represented by $S\phi(x)$.

The integral equations may then be put under the form

$$(1) \qquad a\phi(x) + \lambda S\phi(x) = f(x),$$

where $f(x)$ is a given function; the equation is said to be of the first kind if the coefficient a is null, and of the second kind if this coefficient is equal to 1.

The relation (1) should be satisfied by all the values of y comprised in the field of integration; it is therefore equivalent to a *continuous* infinity of linear equations.

Fredholm has treated the case of the equations of the second kind; the solution then may be put under the form of the quotient of two expressions analogous to determinants and which are integral functions of λ. For certain values of λ, the denominator vanishes. We then can find functions $\phi(x)$ (called *proper functions*) which satisfy the equation (1) when we replace $f(x)$ in it by 0.

The result supposes that the kernel $K(x, y)$ is limited; if it is not, we are led to consider *reiterated* kernels; if we repeat n times the linear substitution S, we obtain a substitution of the same form with a different kernel $K_n(x, y)$; if one of these reiterated kernels be limited this suffices for the method to remain applicable by means of a very simple artifice. Now this happens in a great number of cases, as Fredholm has shown. The generalization for the case where the unknown function depends upon several variables and for that where there are several unknown functions is made without difficulty.

Fredholm then applied his method to the solution of Dirichlet's problem and to that of a problem in elasticity, thus showing how we may attack all questions of mathematical physics.

Such is the part of the first inventor; what now is Hilbert's? Consider first a finite number of linear equations; if the determinant of these equations is symmetric, their first members may be regarded as the derivatives of a quadratic form, and hence results for equations of this form a series of propositions very worthy of interest and well known to geometers. The corresponding case for integral equations is that where the kernel is symmetric, that is to say, where

$$K(x, y) = K(y, x).$$

This Hilbert takes hold of. The properties of quadratic forms of a finite number of variables may be generalized so as to apply to integral equations of this symmetric form. The generalization is made by a simple passing to the limit; but this passing presented difficulties which Hilbert overcame by a method admirable in its

simplicity, certainty and generality. The developments reached are *uniformly* convergent, but this uniformity presents itself under a new form which deserves to attract attention. In the developments appears an arbitrary function $u(x)$ (or several) and the remainder of the series when n terms have been taken is less than a limit depending only upon n and independent of the arbitrary function, provided this function is subject to the inequality

$$\int u^2(x)\,dx < 1,$$

the integral being taken between suitable limits. This is an entirely new consideration which may be utilized in very different problems.

Thus Hilbert obtains in a new way certain of Fredholm's theorems; but I shall stress above all the results which are most original.

In the first place, the denominator of Fredholm's expressions is a function of λ admitting only real zeroes, and this is a generalization of the elementary theorem relative to "the equation in S." Afterward comes a formula where enter under the sign $\int$ two arbitrary functions $x(s)$ and $y(s)$ which we should consider as the generalization of the elementary formulas which permit the breaking up of a quadratic form into a sum of squares.

But I hasten to reach the question of the development of an arbitrary function proceeding according to proper functions. Is this development, the analogue of Fourier's series or of so many other series playing a principal rôle in mathematical physics, possible in the general case? The sufficient condition that a function be capable of such development is that it can be put in the form $Sg(x)$, $g(x)$ being continuous. This is the final form of the resultant as Hilbert gives it in his fifth communication. In the first he was forced to impose certain restrictions; here we must mention the name of Schmidt, who in the interval had produced a work which helped Hilbert to free himself from these restrictions. The only condition imposed upon our function is capability of being put in the form $Sg(x)$, and at first blush this would seem sufficiently complex, but in a large number of cases and, for example, if the kernel is a Green's function, it only requires that the function possess a certain number of derivatives.

Hilbert was afterward led to develop his views in the following manner: he this time considers a quadratic form with an infinite number of variables and he studies its orthogonal transformations; this is as if he wished to study the different forms of the equation of a surface of the second degree in space of an infinite number of dimensions when referred to different systems of rectangular axes. To this effect he makes what he calls the resolvent form of the given form. Let $K(x)$ be the given form, $K(\lambda, x, y)$ the resolvent form sought; it will be defined by the identity

$$K(\lambda, x, y) - \tfrac{1}{2}\lambda\Sigma_r \frac{dK(x)}{dx_r}\frac{dK(\lambda, x, y)}{dx_r} = \Sigma x_r y_r.$$

When the form $K(x)$ depends only upon a finite number of variables, the resolvent form presents itself as the quotient of two determinants which are integral polynomials in λ.

Our author applies to this quotient the procedures of passing to the limit which are familiar to him; the limit of the quotient exists even when those of the numerator and of the denominator do not exist.

In the case of a finite number of variables, $K(\lambda, x, y)$ is a rational function of λ and this rational function can be broken up into simple fractions. What becomes of

this decomposition when the number of variables becomes infinite? The poles of the function rational in λ may in this case or otherwise tend toward certain limit points infinite in number but discrete.

The aggregate of these points constitutes what our author calls the *discontinuous spectrum* of his form. They may also admit as limit points all the points of one or several sects of the real axis. The aggregate of these sects constitutes the *continuous spectrum* of the form.

The simple fractions corresponding to the discontinuous spectrum will make in their totality a convergent series; those corresponding to the continuous spectrum will change at the limit into an integral of the form

$$\int \frac{\sigma d\mu}{\lambda - \mu},$$

where the variable of integration μ is varied all along the sects of the continuous spectrum, and where σ is a suitable function of μ. The rational function $K(\lambda, x, y)$, therefore, has then as limit not a meromorphic function, but a uniform function with erasures. The decomposition into simple elements thus transformed remains valid. If the given form is *limited,* that is to say, if it can not pass a certain value when the sum of the squares of the variables is less than 1, we can deduce thence a way of simplifying this form by an orthogonal transformation, analogous to the simplification of the equation of an ellipsoid by referring this surface to its axes.

Among the quadratic forms we shall distinguish those which are *properly continuous* (vollstetig), that is to say, those whose increment tends toward zero when the increments of the variables tend simultaneously toward zero in any way. Such a form does not have a continuous spectrum and hence result noteworthy simplifications in the formulas.

Other theorems on the systems of simultaneous quadratic forms, on bilinear forms, on Hermite's form, extend likewise to the case of an infinite number of variables.

There was in this theory the germ of an extension of Fredholm's method to kernels to which the analysis of the Swedish geometer was not applicable, and scholars of Hilbert should bring out this fact. However that may be, Hilbert first applied himself to extending his way of looking at integral equations to the cases where the kernel is unsymmetric. For this purpose he introduces any system of orthogonal functions, conformably to which it is possible to develop an arbitrary function by formulas analogous to that of Fourier. In place of an unknown function, he takes as unknowns the coefficients of the development of this function; an integral equation can thus be replaced by a system of a *discrete* infinity of linear equations between a *discrete* infinity of variables.

The theory of integral equations is thus attached, on the one hand, to the ideas of von Koch on infinite determinants, and, on the other hand, to the researches of Hilbert we have just analyzed and where the essential rôle is played by functions dependent upon a discrete infinity of variables.

To each kernel will correspond thus a bilinear form dependent upon an infinity of variables. If the kernel is symmetric, this bilinear form is symmetric and may be regarded as derived from a quadratic form. If the kernel satisfies the conditions stated by Fredholm, we see that this quadratic form is properly continuous and consequently does not have a continuous spectrum. This is a way of reaching Fredholm's results, and however indirect it may be, it opens entirely new views of the profound reasons for these results and hence on the possibility of new generalizations.

Integral equations lend themselves to the

solution of certain differential equations whose integrals are subject to certain conditions as to the limits, and this is a very important problem for mathematical physics. Fredholm solved it in certain particular cases and Picard generalized his methods. Hilbert made a systematic study of the question.

Consider an integral equation

$$\Delta u = f,$$

where u is an unknown function of one or several variables, f a known function and Δ any linear differential expression. This equation with the same right as an integral equation may be considered as an infinite system of linear equations connecting a continuous infinity of variables, as a sort of linear transformation of infinite order, enabling us to pass from f to u. If we solve this equation, we find

$$u = Sf,$$

$S(f)$ this time presenting itself under the form of an integral expression.

Then Δ and S are the symbols of two linear transformations of infinite order inverse one to the other. The kernel of this integral expression $S(f)$ is what we call a *Green's function*. This function was first met in Dirichlet's problem, then it was Green's function properly so called, too familiar to be stressed; we had already obtained different generalizations of it. To have given a complete theory belongs to Hilbert. To each differential expression Δ, supposed of the second order and of elliptic type, to each system of conditions as to the limits, corresponds a Green's function. We cite the formation of the Green's functions in the case where we have only one independent variable and where they present themselves under a particularly simple form, and the discussion of the different forms the conditions as to the limits may assume. That settled, suppose we have solved the problem in the case of an auxiliary differential equation differing little from that proposed and anyhow not differing from it by the terms of the second order; we can then by a simple transformation reduce the problem to the solution of a Fredholm equation where the rôle of kernel is played by a Green's function relative to the auxiliary differential equation. However, the consideration of this auxiliary equation, the necessity of choosing it and solving it being capable of still constituting an embarrassment, in his sixth communication Hilbert frees himself from it. The differential equation is transformed into a Fredholm equation where the rôle of kernel is played by a function our author calls *parametrix*. It is subject to all the conditions defining Green's function, one alone excepted, the most troublesome, it is true; it is not constrained to satisfy a differential equation; it remains therefore in a very large measure arbitrary. The transformation undergone by the differential equation is comparable to that experienced by a system of linear equations if we replace the primitive variables by linear combinations of these variables suitably chosen. The method is nowise restricted to the case where the differential equation considered is adjoint to itself.

Hilbert examined in passing a host of questions relative to integral equations and showed the possibility of their application in domains the most varied. For example, he extended the method to the case of a system of two equations of partial derivatives of the first order of the elliptic type, to *polar* integral equations, that is to say, where the coefficient a in the integral equation (1) in place of being always equal to 1 is a function of x and in particular is equal now to $+1$, now to -1.

He has applied the method to the problem of Riemann for the formation of func-

tions of a complex variable subject to certain conditions as to the limits, to the theorem of oscillations of Klein, to the formation of fuchsian functions, and in particular to the following problem: to determine λ so that the equation

$$\frac{d}{dx}\left[(x-a)(x-b)(x-c)\frac{dy}{dx}\right]+(x+\lambda)y=0$$

may be a fuchsian equation.

One of the most unexpected applications is that Hilbert makes to the theory of the volumes and surfaces of Minkowski, and by which he connects with Fredholm's method a question important for those who interest themselves in the philosophic analysis of the fundamental notions of geometry.

DIRICHLET'S PRINCIPLE

We know that Riemann with a stroke of the pen proved the fundamental theorems of Dirichlet's problem and conformal representation, grounding himself on what he called Dirichlet's principle; considering a certain integral depending upon an arbitrary function U, and which we shall call Dirichlet's integral, he showed that this integral can not become null and from this he concludes that it must have a minimum, and that this minimum can be reached only when the function U is harmonic. This reasoning was faulty, as has since been shown, because it is not certain that the minimum can be actually reached, and if it is, that it can be for a continuous function.

Yet the results were exact; much work has been done on this question; it has been shown that Dirichlet's problem can always be solved, and it actually has been solved; it is the same with a great number of other problems of mathematical physics which formerly would have seemed attackable by Riemann's method. Here is not the place to give the long history of these researches; I shall confine myself to mentioning the final point of outcome, which is Fredholm's method.

It seemed that this success had forever cast into oblivion Riemann's sketch and Dirichlet's principle itself. Yet many regretted this; they knew that thus we were deprived of a powerful instrument and they could not believe that the persuasive force which in spite of all Riemann's argument retained, and which seemed to rest upon I know not what adaptation of mathematical thought to physical reality, was actually only a pure illusion due to bad habits of mind. Hilbert wished to try whether it would not be possible, with the new resources of mathematical analysis, to turn Riemann's sketch into a rigorous proof. See how he arrived at it; consider the aggregate of functions U satisfying proposed conditions; choose in this aggregate an indefinite series of functions S, such that the corresponding Dirichlet integrals tend in decreasing toward their lower limit. It is not certain that at each point of the domain considered this series S is convergent; it might oscillate between certain limits. But we can in S detach a partial series S_1 which is convergent at a point M_1 of the domain; in S_1, detach another partial series S_2 which shall always be convergent at M_1, but which, moreover, shall also be convergent at M_2. So continuing, we shall obtain a series which will be convergent at as many points as we wish; and by a simple artifice we from this deduce another series which will be convergent at all the points of a countable assemblage, for example at all the points whose coordinates are rational. If then we could prove that the derivatives of all the functions of the series are less in absolute value than a given limit, we might conclude immediately that the series converges uniformly in the whole domain and the application of the rules of the calculus

of variations would no longer present special difficulty.

To establish the point remaining to be proved, Hilbert has used two different artifices; he has not developed the first as completely as would be desirable, and has attached himself especially to the second. This consists in replacing the proposed function u by the function v, which comes from it by a double quadrature and of which it is the second derivative with regard to two independent variables. The derivatives of v being the first integrals of u, we can assign them an upper limit, by the help of certain inequalities easy to prove. Only it is necessary to be resigned to a new circuit and to an artifice simple however to apply to this new unknown function v the rules of the calculus of variations which apply so naturally to the function u.

It is needless to insist upon the range of these discoveries which go so far beyond the special problem of Dirichlet. It is not surprising that numerous investigators have entered the way opened by Hilbert. We must cite Levi, Zaremba and Fubini; but I think we should signalize before all Ritz, who, breaking away a little from the common route, has created a method of numeric calculus applicable to all the problems of mathematical physics, but who in it has utilized many of the ingenious procedures created by his master Hilbert.

Recently Hilbert has applied his method to the question of conformal representation. I shall not analyze this memoir in detail. I shall confine myself to saying that it supplies the means of making this representation for a domain limited by an infinite number of curves or for a simply connected Riemann surface of an infinity of sheets. This therefore is a new solution of the problem of the uniformization of analytic functions.

DIVERS

We have passed in review the principal research subjects where Hilbert has left his trace, those for which he shows a sort of predilection and whither he has repeatedly returned; we must mention still other problems with which he has occupied himself occasionally and without insistence. I think I should confine myself to giving in chronologic order the most striking results he has obtained of this sort.

Excepting the binary forms, the quadratic forms and the biquadratic ternary forms, the definite form most general of its degree can not be broken up into a sum of a finite number of squares of other forms.

By elementary procedures may be found the solutions in integers of a diophantine equation of genus null.

If an integral polynomial depending upon several variables and several parameters is irreducible when these parameters remain arbitrary, we may always give these parameters integral values such that the polynomial remains irreducible.

Consequently there always exist equations of order n with integral coefficients and admitting a given group.

The fundamental theorem of Dedekind about complex numbers with commutative multiplication may be easily proved by means of one of the fundamental lemmas of Hilbert's theory of invariants.

The diophantine equation obtained by equating to ± 1 the discriminant of an algebraic equation of degree n has always rational solutions, but save for the second and the third degrees has no integral solutions.

Among the real surfaces of the fourth order, certain forms logically conceivable are not possible; for example, there can not be any composed of twelve closed surfaces simply connected or of a single surface with eleven perforations.

CONCLUSIONS

After this recital, a long commentary would be useless. We see how great has been the variety of Hilbert's researches, the importance of the problems he has attacked. We shall signalize the elegance and the simplicity of the methods, the clearness of the exposition, the solicitude for absolute rigor. In seeking to be perfectly rigorous one risks at times being long, and this is not to buy too dear a correctness without which mathematics would be nothing. But Hilbert has known how to avoid the tedium of such diffuseness for his readers in never letting them lose from view the guiding thread which has served him to orient himself. We always easily see by what chain of ideas he has been led to set himself a problem and find its solution.

We realize that, more analyst than geometer in the ordinary sense of the word, he nevertheless has seen at one view the totality of his work before distinguishing details and he knows how to give his reader the advantage of this all-embracing vision.

Hilbert has had a tremendous influence upon the recent progress of the mathematical sciences, not alone by his personal work, but by his teaching, by the counsel he has given to his scholars and which has enabled them to contribute in their turn to this development of our knowledge by using the methods created by their master.

There is no need, so it seems, to say more in justification of the decision of the commission which has unanimously awarded to Hilbert the Bolyai prize for the period 1905–1909.

M. Poincaré

THE WILLARD GIBBS MEDAL

In the early part of 1909 Mr. William Converse, of Chicago, proposed to the Chicago Section of the American Chemical Society to found a gold medal to be awarded annually by the Section. Mr. Converse stated that the object of his proposition was to stimulate interest in the work of the Section and of the society at large and to encourage the highest ideals of the science in their members. The Section gladly welcomed and accepted the offer made. It was proposed to name the medal after the most eminent chemist America has given to the science, and the consent of Mrs. Van Name, the surviving sister of Willard Gibbs, having been secured, the medal founded by Mr. Converse was named the Willard Gibbs Medal. After various plans had been suggested and discussed, the Section decided that the medal should be awarded annually, by invitation, rather than by competition and the following rules were adopted for the award.

RULES FOR THE AWARD OF THE WILLARD GIBBS MEDAL, FOUNDED BY WILLIAM A. CONVERSE

1. A gold medal shall be awarded annually by the Chicago Section of the American Chemical Society at its May meeting, which meeting shall be open to the public.

The medal is to be known as the Willard Gibbs Medal founded by William A. Converse.

The award shall be made according to the rules here set forth and made a part of the by-laws of the Chicago Section.

2. The award shall be made by a two-thirds vote of a jury of twelve, to anybody who because of his eminent work in and original contributions to pure or applied chemistry, is deemed worthy of special recognition by the jury.

3. A condition of the award shall be that the recipient of the medal shall deliver an address upon a chemical subject of his own selection and satisfactory to the jury at the May meeting of the Chicago Section of the American Chemical Society. He shall be notified of the award three months in advance of this meeting by the chairman of the Chicago Section.

4. The jury of the award, to be known as the Jury of the Willard Gibbs Medal, shall consist of twelve members, six of them to be members of the Chicago Section. The chairman of the Chicago Section shall be chairman of the jury, but shall have no vote.

5. Four members of the jury shall be elected each year to serve three years, in the same manner

and at the same time as the officers of the Chicago Section.

At the first election of the jurors of the Willard Gibbs Medal, to be held in 1911, four jurors shall be elected to serve a term of one year, four to serve a term of two years and four to serve a term of three years. Of each four elected, two shall be from the Chicago Section.

6. At the call of the chairman of the Chicago Section the jury shall begin its deliberation on January 2 of each year.

Each member of the jury shall be entitled to place in nomination the names of two candidates. The voting shall then be on these candidates.

The four names receiving the highest number of votes on the first ballot shall be retained, the others rejected.

If of the four names retained, none receives a two-thirds vote on the second ballot, the two receiving the fewest votes shall be dropped. If on further balloting the committee finds it impossible to make a selection by a two-thirds vote, it will report to the section, which will proceed to elect the recipient of the medal; but if any candidate receives a two-thirds vote of the committee, his election shall be final and shall be so reported to the section.

7. It is desired that the paper or address, if suitable, be published in one of the publications of the American Chemical Society.

8. The executive committee of the Chicago Section shall have the power to decide any question not specifically covered by these rules.

9. The Chicago Section shall have the power to change or amend these rules in the same manner as the by-laws of the section.

For the first year of the foundation, 1911, by special amendment of the rules of the Section a special jury of award was elected, consisting of the following members: S. A. Mather, chairman of the section and president of the Thorkildsen-Mather Co.; W. Brady, chief chemist of the Illinois Steel Co.; D. K. French, secretary of the section and chemist of the Dearborn Drug and Chemical Co.; W. Hoskins, of Mariner and Hoskins; Professor John H. Long, of the Northwestern University Medical School; A. Lowenstein, chief chemist of Nelson Morris & Co.; Professor H. McCormick, of Armour Institute; Professor H. N. McCoy, of the University of Chicago; W. D. Richardson, chief chemist of Swift & Co.; Professor Alexander Smith, of the University of Chicago, and president of the American Chemical Society, and Professor Julius Stieglitz, of the University of Chicago. By a unanimous vote the jury decided to award the first medal to Professor Svante Arrhenius for his fundamental work on the theory of electrolytic dissociation.

The medal was presented to Dr. Arrhenius on the evening of May 12, after a banquet which was attended by over 200 members and guests of the section. The formal program of the evening included the following addresses: "International Bonds of Science," by Harry Pratt Judson, president of the University of Chicago; "Chemistry and Commerce," by Mr. Wheeler, president of the Association of Commerce of Chicago; "The Willard Gibbs Medal," by S. A. Mather, chairman of the Chicago Section of the American Chemical Society; "The Presentation of the Willard Gibbs Medal to Dr. Arrhenius," by Alexander Smith, president of the American Chemical Society, and "The Willard Gibbs Address," by the medallist, Svante Arrhenius, on "The Theory of Electrolytic Dissociation." The last address gave, in outline, the history of the discovery of the theory of electrolytic dissociation; it formed, on the one hand, an intensely interesting record of the birth of a great idea and theory, of its early difficulties and its final triumph; and, on the other hand, it presented a picture of the struggles, progress and development of the genial discoverer of the theory.

The address will be published under the auspices of the Chicago Section of the American Chemical Society.

SCIENTIFIC NOTES AND NEWS

Dr. Samuel H. Scudder, of Cambridge, eminent for his contributions to entomology, especially lepidoptera and fossil insects, died on May 17, aged seventy-four years.

During his recent visit to Washington at the time of the annual meeting of the National Academy of Sciences, Sir John Murray presented a fund of six thousand dollars to

the academy for the purpose of founding an Alexander Agassiz gold medal which shall be awarded to scientific men in any part of the world for original contributions to the science of oceanography.

At the twentieth annual commencement of Stanford University, to be held from May 17 to 22, a portrait of President Jordan will be presented to the university.

Colonel William Gorgas, U.S.A., head of the sanitary forces on the Isthmus of Panama, received the honorary degree of doctor of laws from Tulane University at its annual commencement on May 17.

Professor Paul H. Hanus, head of the department of education at Harvard University, has been chosen to take general charge of the investigation of the New York public school administration conducted by the School Inquiry Committee.

Professor C. F. Mabery has resigned the professorship of chemistry in Case School of Applied Science, which he has occupied since 1883.

Dr. J. Rein, professor of geography at Bonn, has celebrated the fiftieth anniversary of his doctorate.

Dr. R. Fick, professor of anatomy at Innsbruck, has been elected a corresponding member of the Royal Society of Physicians of Vienna.

The Pharmaceutical Society has elected the following honorary members: Professor W. E. Dixon, F.R.S., professor of pharmacology, King's College, London; Dr. Adolph Engler, director, Botanical Museum, Berlin; Professor Percy F. Frankland, F.R.S.; M. Eugène Léger, pharmacien en chef de l'Hôpital St. Louis, Paris; Lieutenant-Colonel D. Prain, F.R.S., director of Royal Gardens, Kew; and Dr. Ludwig Radlkofer, professor of botany, University of Munich.

The *Bulletin* of the American Mathematical Society states that the eminent mathematician, Professor Gaston Darboux, of the University of Paris, being about to complete his fiftieth year of service as a teacher in the system of public instruction of France, it is proposed by a large international group of his mathematical co-workers, friends and former pupils to commemorate this anniversary by presenting to Professor Darboux a gold medal bearing his portrait, and an appropriate address signed by the participants. All mathematicians are invited to share in rendering this honor to Professor Darboux. Copies of the medal, in reduced size, will be struck. Subscribers of twenty-five francs will receive a copy in bronze, subscribers of fifty francs a copy in silver. Subscriptions should be sent to Professor Cl. Guichard, secretary of the Faculté des Sciences.

Dr. Johannes Hartmann, professor of astronomy at Göttingen and director of the university observatory, has been called to be director of the Argentine Observatory at La Plata.

Professor C. H. Hitchcock, emeritus professor of geology at Dartmouth College, has come east from Hawaii for the purpose of completing his field work for the Geological Survey of Vermont. Some attention will also be paid by him to ichnological studies. His address will be at Hanover, N. H., for the summer.

Professor Alexander Graham Bell returned on May 8 from a trip around the world.

Dr. T. C. Mendenhall, formerly of the Ohio State University and later president of Worcester Polytechnic Institute, is on a visit to Japan, where from 1878 to 1881 he occupied the chair of physics in Tokyo University.

Dr. John C. Branner, of Stanford University, is the head of a scientific expedition to the coast of Brazil, which sailed from New York on April 18 for Para.

Mr. Wilfred H. Osgood, of the Field Museum of Natural History, has returned from three months work in Venezuela and Columbia, having obtained important collections of birds and mammals, including a small series of the rare marsupial, *Cænolestes*, a living representative of the family Epanorthidæ.

WE learn from the *Auk* that Mr. A. C. Bent, of Taunton, Mass., is organizing an expedition to the Aleutian Islands for the purpose of making a thorough biological survey of that interesting region, covering practically the whole of the summer season. Negotiations are now on foot to secure the use of a revenue cutter to take the party, which will consist of three scientific men in addition to Mr. Bent. Mr. Rollo H. Beck, known for his work in the Galapagos Islands and along the coast of California, has already been engaged, and it is probable that the United States National Museum and the Biological Survey will each send a representative.

PROFESSOR W. F. WATSON, who has held since 1890 the chair of chemistry and biology at Furman University, Greenville, S. C., has resigned, and will spend four years in a tour around the world.

DR. SVANTE ARRHENIUS lectured at the College of the City of New York on May 17 and at Columbia University on May 18. On May 15 he lectured on the J. C. Campbell foundation of the Sigma Xi Society of the Ohio State University.

DR. WALTER B. CANNON, professor of physiology in the Harvard Medical School, will give the annual address before the graduating class of the Yale Medical School at the approaching commencement.

PROFESSOR JOHN M. COULTER, head of the department of botany of the University of Chicago, will give an address before a joint meeting of the Sigma Xi and Phi Beta Kappa fraternities on June 12, as a part of the program of commencement week at the University of Illinois.

PROFESSOR W. W. OSTERHOUT addressed the Biological Society of Smith College on May 18 on "Some Aspects of the Action of Mineral Salts on Plants."

DR. E. E. BARNARD, of Yerkes Observatory, lectured on "Photographic Revelations in Astronomy" before the Dayton Astronomical Society on May 10 and before the Cincinnati Astronomical Society on May 12.

AT the first annual meeting of the Cincinnati Society, held May 12, the following officers were elected: Dr. Lisle Stewart, president; W. C. Cooder, vice-president; Robert H. Correy, secretary; A. D. Fisher, treasurer; A. D. Alcorn, P. B. Evens, J. D. Griese, A. P. Henkel, C. H. Norton, M. C. Slutes, directors. President Taft was unanimously elected an honorary member. Sixty-five men and women joined as charter members. This society expects to interest itself particularly with astronomical and astrophysical research.

WE learn from *Nature* that a committee of the Geological Society, London, has been formed to secure the means of providing a memorial to the late Professor T. Rupert Jones, F.R.S., in aid of his widow and daughters. The late Professor Jones was never in receipt of more than a very moderate income, and received only a small pension upon his retirement thirty years ago from the post of professor of geology in the Royal Military College, Sandhurst.

THE ninety-fourth annual meeting of the Swiss Scientific Society, will be held this year at Solothurn, from July 30 to August 2, under the presidency of Dr. A. Pfaehler. In addition to the general sessions for which a number of addresses of general interest are arranged, there meet with the association the Swiss societies for botany, chemistry, geology, mathematics, physics and zoology. Foreign men of science are especially invited to be present at the meetings.

THE H. F. KIETH COMPANY, of Boston, have given $5,000 to the Massachusetts Institute of Technology, for a research on the decomposition and general wholesomeness of eggs and for an investigation of the bacterial and chemical contents of the product under varying conditions.

Two collections of birds have been placed on deposit in the American Museum of Natural History. One of these, the property of Dr. Jonathan Dwight, Jr., of New York City, numbers about 30,000 specimens, ranking as one of the largest private collections in this country. It is especially valuable in showing

plumages and molts of North American species. The second collection belonging to Dr. Leonard C. Sanford, of New Haven, Connecticut, contains about 400 specimens, largely non-passerine birds, and includes rare species especially among the albatrosses and petrels, some of which are not represented in the American Museum collections.

AN arrangement has been concluded between the German and English governments and the Marconi Company by which the weather observations transmitted by wireless telegraphy from ships on the Atlantic will be made mutually available to the English and German Meteorological Offices. Experiments in this direction were made in 1909. The new arrangement is expected to come into force by next year at latest. The observations will be transmitted to the Meteorological Office in London, to the Marine Observatory at Hamburg and to the Meteorological Station at Aachen.

IN Bulletin 420 of the United States Geological Survey, entitled "Economic Geology of the Feldspar Deposits of the United States," by Edson S. Bastin, there are descriptions of the many feldspar deposits in the country and the extent to which the industry has grown. The principal consumers of feldspar are manufacturers of pottery, enamel ware, enamel brick and electric ware. The trade demands that feldspar for use in pottery be nearly free from iron-bearing minerals (biotite, garnet, hornblende, black tourmaline, etc.) and that it contain little if any muscovite. Feldspar is also used in the manufacture of emery and carborundum wheels, as a flux to bind the abrading particles together. Small quantities of feldspar are used in the manufacture of opalescent glass and carefully selected pure feldspar is used in the manufacture of artificial teeth. Some is used in scouring soaps and window washes, the fact that feldspar is slightly softer than glass rendering these soaps less liable to scratch windows or glassware than the soaps in which quartz is the abrasive substance. Two firms in New York and one in Connecticut crush feldspar for poultry grit and for use in the manufacture of ready roofing. In a number of the feldspar quarries garnets, green tourmalines and aquamarines (beryl) of gem quality are found, but seldom in such quantity as to warrant mining for the gems alone. Mr. Bastin mentions a feldspar quarry in Connecticut where some of the cavities that yielded gem tourmalines were as large as a bushel basket. At another quarry in the state a large transparent green tourmaline about seven inches long was found. This stone is now in the museum of the Wesleyan University at Middletown, Conn. One pocket in the same quarry contained a large crystal weighing several pounds, of pale-blue to pale-green color, the tints being similar to those observed in some aquamarines. Unfortunately, this crystal was much shattered in the blasting, but the fragments have yielded a number of small cut gems of great beauty.

UNIVERSITY AND EDUCATIONAL NEWS

GOVERNOR Foss has signed the bill by which the Massachusetts Institute of Technology will receive $100,000 annually from the state for ten years. By the terms of the measure the Institute will maintain 80 free scholarships to be apportioned among the 40 senatorial districts of the state.

THE California legislature has passed a bill which has been recently signed by the governor appropriating $25,000 for a soils laboratory building, equipment and other improvements at the Citrus Experiment Station. About $1,500 of this amount will be used in improving the irrigation system, $2,500 to complete the title for building site and nursery grounds, about $2,000 for incidentals, leaving $19,000 for building and equipment. The work of this laboratory is to be confined to the study of citrus soils from their chemical, physical and biological phases.

THE legislature of Hawaii, just adjourned, appropriated $75,000 for a new building for the College of Hawaii and $20,000 for maintenance expenses. The committee of education favored the adoption of the plans that have been drawn up for the development and

embellishment of the campus and grounds. These grounds are located in Manoa, a suburban valley with both mountain and sea views, and comprise about ninety acres. Sixty acres were purchased and thirty acres were set aside by the government. The total grounds with its water has a market value of about $125,000.

M. ALBERT KAHN, of Paris, who has established traveling fellowships in several foreign countries, has given $2,500 for such a fellowship in the United States. It is expected that the fellow selected will travel around the world giving a year to the trip. Selection of the fellow will be made by the trustees, who are Edward D. Adams, Nicholas Murray Butler, Charles W. Eliot, Henry Fairfield Osborn and Charles D. Walcott, and they are to choose preferably professors in isolated southern and western institutions.

DR. H. Y. BENEDICT, professor of applied mathematics and director of the department of extension of the University of Texas, has been made dean of the College of Arts.

AT the University of Pennsylvania Dr. Richard M. Pearce has been transferred from the chair of pathology to that of experimental pathology, and Dr. Allen J. Smith has been transferred from the chair of tropical diseases to that of pathology, formerly occupied by him.

DR. LUTHER WILLIAM BAHNEY, assistant professor of metallurgy at Leland Stanford University, has been appointed assistant professor of mining and metallurgy in the Sheffield Scientific School, Yale University.

DR. CLARENCE A. PIERCE, of Cornell University, has been appointed assistant professor of theoretical electrical engineering at the Worcester Polytechnic Institute to succeed Dr. George R. Olshausen, who has resigned after four years of service.

DR. WALTER S. TOWER, assistant professor of geography in the University of Pensylvania, has been called to the University of Chicago.

DR. J. FRANK DANIEL has been promoted to be assistant professor of zoology in the University of California.

DISCUSSION AND CORRESPONDENCE

THE LAW THAT INHERES IN NOMENCLATURE

DR. JORDAN'S answer[1] to my inquiry,[2] "Whether there is not a better way of disposing of our nomeclatural trouble than first making it as burdensome as possible and then making it permanent?" is, if I understand him aright, that, alas, there is none; at least, there is none yet in sight, or likely to appear. Hence it were better to take up the burden cheerfully, and begin getting used to it.

Whether one be pleased with this prospect or not, he must be grateful for Dr. Jordan's clear and forceful statement of certain guiding principles. This, for example, seems to me to go to the heart of the matter under discussion:

"A writer dealing with scientific names must either call an animal or plant what he pleases, or else he must conform to regulations inherent in the nature of his work. Arbitrary rules will soon be disregarded. The necessary regulations are those which future workers will approve, and we who are working in the infancy of taxonomy must lay foundations on which the future can build." With this we may all agree; though we may hold somewhat different views as to what is the law that inheres in the nature of our work, and as to what rules are arbitrary.

Surely no argument is needed against a return to the loose nomenclatural methods of the past. I protest against the implication that I have advocated anything of the sort. On the contrary, I have advocated the strictest application of the laws that have been evolved by our past nomenclatural experience. I would accept a list of names exactly as furnished by the best historical knowledge that could be brought into service in producing it. And then, because such a system would be more than human nature can bear, more than language can use, and more than our science can make its best progress under, I would provide for general use a terminology giving expression to the same system in simpler form, with fewer, briefer and simpler names, and

[1] SCIENCE, March 10, 1911.

[2] SCIENCE, September 2, 1910.

symbols. That is the whole of it. No plan for solving zoological problems by rule is proposed; only a plan for conserving time and energy, offered in the belief that the purely clerical work of biological science might be accomplished with less waste. The simpler system would stand in the same relation to the existing system as that in which the Linnæan names have stood to the long descriptive phrases that preceded them.

To be sure, this plan, which allows choice of names (one out of a score more or less in every group), does not necessitate that the oldest one shall be forced into general use in the new system: rather, it leaves the selection to those most competent, most interested and most responsible for the future in each group. This feature may hold the derogation of democracy to which Dr. Jordan refers, but if so, I do not understand what sort of a democracy systematic zoology is considered to be. Is a law of priority its only possible standard of equality? I profess to be a democrat, and, in a very small way, a systematist; yet I confess I never heard of anything like this. May not this democracy abide the recognition of merit? Is it already irrevocably bound up with a statute of nomenclatural primogeniture? Does the determination of priority in and of itself necessitate that all good democrats must acclaim the restoration of lost names to the places they once transiently occupied in spite of all that may have happened in the intervening years?

I have myself long pursued priority in the hope of names that would be both stable and usable. I have even advocated the forcing of prior forgotten names back into general nomenclature. I did so as long as mere temporary convenience seemed at stake. I did so while names doubled in length, trebled in absurdity and quadrupled in number. I did so until family names began to fall and to be set up again in exchanged places. I did so until I became unable to read the literature in several groups of which I had once been a student, or to converse with modern students of those groups. I did so until it became well nigh impossible for me to give to my classes intelligible references to the literature they most needed to consult in their work.[3] And

[3] Recently, while providing tables for the work of a small class in limnology, I encountered the following situation in aquatic diptera. Half of the names of dipterous families containing aquatic larvæ have been victims of the rule of priority. Here are the names of the families of our fauna, as found in all the text-books, manuals, monographs and general reference books.

Psychodidæ	* Leptidæ
* Ptychopteridæ	Empididæ
Tipulidæ	x * Stratiomyiidæ
x * Blepharoceridæ	Syrphidæ
Dixidæ	* Borboridæ
* Chironomidæ	Ephydridæ
Culicidæ	x * Cordyluridæ or * Scatophagidæ
* Simuliidæ	
Tabanidæ	Sciomyzidæ

Only those unmarked in the list remain unchanged. Of the others, three (marked x) have been changed in spelling only, return to an incorrect or inelegant form being required in this line of progress. One of these names, Cordyluridæ, is in less common use than Scatophagidæ, but Scatophaga also falls. In addition to this, the well-known names *Syrphus* and *Sciomyza* have been shifted to designate new groups of species in their respective families. So, also, has *Corethra* within its subfamily. All these familiar groups will now bear unfamiliar names.

Now, perhaps, a better democrat than I would have adopted all these changes willingly and pursued priority to the bitter end. But I did not. I wished my class to use the literature that has grown up about the names *Corethra, Chironomus, Simulium, Eristalis,* etc., names that are the subjects of books, of memoirs and of classic investigations in many fields of biology, and that have nowhere any uncertain meaning. As a teacher I could not afford the time and effort necessary to explain to rational young people why the "interests of taxonomy" require that *Corethra* or *Syrphus* be removed from their accustomed places after one hundred years, and used to designate entirely different groups of flies. In fact, I can not explain this; nor why, if the zoologists of the world have been able to agree on a law of priority, they might not yet be able to agree upon something less distressing.

Any one who speaks of this as a matter of *temporary* inconvenience surely is thinking in terms of geologic time.

then I began to entertain doubts as to the approval of posterity, the best kind of foundations, etc. I began to lose faith in the law of priority as a cure-all for nomenclatural ills. For the real burden of nomenclature will be but little altered by the strictest application of this law. At worst (and surely the worst is now in sight) it will have added but a little dead weight of stupid and unnecessary confusion—so little, indeed, it would hardly be noticeable were not the load already at the endurance limit. With all the arduous labor now required of any youth for gaining even an elemental conception of the world's accumulated store of knowledge, why should any man, even though a profound scholar, familiar with the intricacies of his own field, so far forget or minimize the difficulties of the long way by which he has come as to be willing to leave the path harder for the next comer. Ought not the way that leads to a working knowledge of plants and animals to be as easy and plain as we can possibly make it? I think so. And so thinking, I ventured to propose, after long consideration, the simplification that is now under discussion.

My plan would accept the facts of nature as they are—exceedingly complicated. They are not more complex under one system than under another. And it is a great error to assume that because facts are numerous and relations complex, the method of handling them must be equally so.

My plan would accept human nature as it is—exceedingly prone to differences of opinion; yet, withal, able often to agree upon such matters as dates of publication.

My plan would accept the results of the application of the law of priority *in toto,* conserving all the good work that has been done by the zoologists of the world in their search of early literature. It would keep the results of this work forever accessible, without making of its by-products stumbling blocks in the way of beginners, of general students, and of the increasing thousands who may have an interest in biological sciences. This work is of great historic value. It is worth while to have all the old and unused names set in their proper order and sequence. But to have any such of them as have lain buried during the growth of a great literature, used when exhumed to replace the names about which that literature has grown, making its treasures less accessible, is a lamentable abuse of the historic method.[4] Let us accept the good work that has been done in determining priority at its historical value, and then let us use it like rational beings for our assistance, without making it a source of embarrassment for future generations.

My plan would accept the Linnæan system as it is, recognizing species as real entities that have received and that will continue to receive names. Were Linnæus resurrected today, he might have difficulty in recognizing his own system, in its present dropsical condition. Those who value it so highly should at least remember that, whatever it has become, it was in the beginning simply and solely an effort at simplification of nomenclature.

The matter of numbering species is so simple it is hard to understand how any difficulty is found in applying it. Given a list of the names now recognized in any group written down in their original form and in their historic sequence, any common clerk could affix the numerals correctly. Their stability would be assured by the only means whereby anything becomes stable—by adoption and use. Any one who will read my proposal with reasonable care will see (1) that it accepts every name exactly as given by its author, and finds a place for it in its proper sequence; (2) that it matters not at all where we begin numbering, and (3) that it matters not at all whether *Balanoglossus* and the tunicates are fishes or not.

I regret Dr. Jordan did not see these things, for then he might have saved space for a statement of the inherent law of nomenclature. Formulation of it is badly needed.

[4] My proposal, however, was to let the principal workers in any group decide upon the names to be used in it. If those who study lancelets do not wish to use the name *Amphioxus,* neither do I wish to use it.

Elsewhere real progress is found in the direction of simplification, which makes for convenience, saves time, and meets the limitations of memory by instituting more concise methods of making records. Does the law that inheres in nomenclature differ so much from that which obtains in all other vast accumulations of facts? If so, let us have a statement of it, so that we may, by understanding it, attain to acquiescence in the inevitable.

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ON EVIDENCE OF SOMA INFLUENCE ON OFFSPRING FROM ENGRAFTED OVARIAN TISSUE

TO THE EDITOR OF SCIENCE: In publication No. 144 of the Carnegie Institution of Washington entitled, "On Germinal Transplantation in Vertebrates," by Castle and Phillips, issued March 14, 1911, an attempt is made to overthrow my experiments on transplantation of ovaries in fowls,[1] and Magnus's[2] experiments of similar character on rabbits, and to establish a claim to priority in the demonstration that offspring may result from transplanted ovaries; and the effect, if any, of soma influence on such offspring. Therefore, I feel it incumbent to call attention briefly to certain of the statements in order that no misunderstanding may result. Since my papers with the experiments are readily available, I shall avoid all unnecessary repetition.

In a word, the situation is as follows: By exchanging the ovaries of fowls and breeding the fowls, I obtained results which seem to show that the transplanted ovaries preserved their reproductive function; and the resulting offspring presented evidence of soma or foster-mother influence. The results are given in detail in my several papers. I may add that since I had no allegiance with any school of theorists, I was not involuntarily partial in observing and recording the results. Whether the results would substantiate either or neither of the theories built largely upon speculation as to the relationship of reproductive tissues to their environment, or whether the character of the offspring would conform to Mendel's results of studies of inheritance in peas, gave me no concern.

The primary object of the experiments was to determine if an engrafted ovary might retain its reproductive function. Therefore, an answer to the question was obtained. And incidentally information on soma influence was secured. Following this, it seemed of additional interest to reverse the matings of the parent stock. And also, by breeding, to study the character of the offspring from the offspring obtained from engrafted ovaries. Unfortunately before this was accomplished, the experiments were terminated by an outbreak of disease among the fowls. But I did not consider then, nor have I since come to believe, that the character of the offspring of the second generation could do more than indicate whether or not soma influence might be evident in the character of the offspring of this generation, that is, the grand chicks. But owing to a degree of familiarity with the general principles of physiological experimentation and interpretation, from the beginning I saw the limitations to the absoluteness of any evidence that might be obtained by continuation of such experiments. For example, before drawing the provisional conclusions in the announcement of my results, the statement was made that "more data must be had on these points before definite conclusions can be drawn."[3] Apparently Castle has

[1] "Results of Removal and Transplantation of Ovaries in Chickens," presented before the American Physiological Society in connection with the seventh meeting of the Congress of American Physicians and Surgeons, Washington, D. C., May 7–9, 1907 (*American Journal of Physiology*, 1907, XIX., xvi–xvii). "Further Results of Transplantation of Ovaries in Chickens," *Journal of Experimental Zoology*, 1908, V., 563. "On Graft Hybrids," presented before the American Breeders' Association, Omaha, December, 1909. "Survival of Engrafted Tissues. I. (A) Ovaries and (B) Testicles," *Journal of Experimental Medicine*, 1910, XII., 269.

[2] Magnus, "Transplantation af Ovarier med Saerligt Hensyn til Afkommet," *Norsk Magazin for Laegevidenskaben*, 1907, No. 9.

[3] *Journal of Experimental Zoology*, June, 1908, V., p. 570.

overlooked this statement. And I may say that all subsequent statements regarding my results have been made from the same standpoint.

In attempting to interpret my results from the Mendelian standpoint, to overcome the difficulty in concluding that in no instance the offspring were derived from engrafted ovarian tissue, Castle can only see his way clear by speculating as to the result that might have followed had I employed two white cocks in the matings, one cock being a half-breed. But he assumes that only one white cock was used, for, as he points out, I use the expression "*the* white rooster." But since a point of doubt has been raised as to whether one or more white cocks were employed, and since Castle claims that I make no specific statement on this point, I would refer to the table on page 565 of the paper appearing in the *Journal of Experimental Zoology,* which is headed "weights of the chickens were as follows," in which the experiment numbers of the individuals, both male and female, used in the experiment are given, together with their weights.

In respect to the evidence of soma influence, this was observed in the offspring directly from the transplanted ovaries. Therefore, it is not open to the same doubt as in the case of more indirect or circumstantial evidence. But supposing that such offspring had been bred, and supposing the offspring resulting from this mating (grand chicks) had or had not presented characteristics indistinguishable from the offspring obtained by straight breeding or of hybrids obtained by crossing unoperated fowls of the breeds employed, such results could not affect the conclusions of foster-mother influence in the first generation. It would only show that in the particular individuals presenting feather markings indicating soma influence, that similar feather markings were or were not transmitted to their offspring, or that individuals presenting no such markings might or might not transmit evidence of soma influence to the next generation. Again, the fact that the markings in all cases were not uniform in the offspring of the first generation, in no way invalidates the results. For all exact knowledge of soma influence must of necessity spring directly from experimental results. Therefore, it can not be assumed that all such offspring must present similar characters either to be acceptable as evidence that an engrafted ovary may preserve its reproductive function, or that such offspring may be influenced by the somatic tissues of the host. That is to say, it is not permissible to assume that all of such offspring would be influenced in the same direction or to the same degree. Nor can it be assumed that evidence of soma influence can be demonstrated in other combinations of fowls, much less in different species of animals.

Seemingly a lack of insight into the underlying physiological principles in such experimentation has led Castle and his collaborator into a misunderstanding, and therefore into stating their belief that my interpretation of the results, and my criticism of a statement of theirs regarding evidence of soma influence,[4] was due to a failure to grasp fully the laws of inheritance of the character which I used as a criterion. But this is more of a personal matter and therefore of no general interest.

These writers call attention to the fact that Davenport attempted to repeat my experiments on fowls, with the result that in every case spaying was incomplete, and the young from such operated hens showed no influence of the introduced graft. This is far from being an argument against the acceptance of my conclusions, as all that it shows from his interpretation is that the ovaries were incompletely removed in his experiments. But as a matter of fact, his experiments and results, while meagerly reported,[5] such as they are, might as well lead to the conclusion that he obtained very strong evidence of soma influence. That is, the chicks so closely re-

[4] "Guinea-pig Graft-hybrids," SCIENCE, N. S., 1909, XXX., 724.

[5] Davenport, "Inheritance of Plumage Color in Poultry," *Proceedings of the Society for Experimental Biology and Medicine,* 1910, VII., 168.

sembled the foster mother that he was led to ascribe the result to original ovarian tissue of the foster mother. This assumption was based upon another assumption, namely, that chicks from the engrafted ovaries would preserve the characters of the fowl from which the ovaries were obtained. The fallacy of this assumption has been pointed out above.

Davenport did not use standard varieties of fowls, so far as I am able to determine from his statements. This is unfortunate, as it is obviously impossible to discuss his findings from the standpoint of relationship of donor to host. For example, I have shown that engrafted ovaries in fowls do not succeed if the stock is too distantly related.

Davenport states that my results justify the opposite conclusions to those which I have drawn; but since he does not give any reasons nor present any evidence for such a conclusion, it carries no weight other than as a personal opinion.

Castle and Phillips ask that my experiments be repeated before they accept my interpretation of the results. In reply, I ask why they did not employ fowls (chickens) in order to confirm or discredit my experiments. I may say that my first series of fowls, operated on in the summer of 1904, were all lost through lack of proper facilities. The next series, operated on in 1906, were given my undivided attention and furnished the material for my papers. A larger series operated on the following year with the view of extending the observations and investigating new fields opened up by the successful series, were not productive of results in the direction of permitting the study of offspring from engrafted ovaries, but furnished considerable information along other lines which is in part presented in my later papers. Successful breeding of fowls, as every one knows, demands the fulfillment of certain requirements in the way of quarters, and facilities for hatching and raising the chicks, and intelligent attention. As to the first two of these requirements, the third series of experiments clearly proves that the quarters and facilities at my disposal, though after a manner adequate for eight fowls, the number composing the second series of experiments, were not adequate for five times this number, the approximate number that were included in the third series. Also, it was not possible for me to give as much time to the third series as to the second. Immediately following this, I made application to the officers of one of the endowed research funds for support in prosecuting the investigation on a much larger scale, which included the employment of a number of species of animals. But for perfectly good reasons the request was denied. Since that time new experiments have been continuously in progress, but they have been designed with a view of keeping within the limits of my facilities.

I do not propose to enter into a discussion of Castle and Phillips's results in this place, save to challenge their assertion that theirs is the first critical case of successful ovarian transplantation from the standpoint discussed above, on record. This statement I make in view of the fundamental considerations also above stated, as well as from an examination of their protocols. For example, they used mongrel stock. Therefore, any evidence furnished by the character of the offspring would be of doubtful value. This is true particularly as regards soma influence; and as crossbreeding was not employed, any evidence of soma influence in the offspring would have been obscured by the character of the male parent.

Also it is not proven that the offspring may not have come from ovarian tissue of the host left in site after operation. Indeed, an interpretation of their results from the numerical standpoint, a criterion employed by them in interpreting their results from the Mendelian standpoint, it would be as fair to conclude that in all of their pigs that became pregnant no post-mortem findings are given. And after operation that this was due to incomplete removal of ovarian tissue. For they state that of the five animals in this group, the results in three were due to ovarian tissues generated from the host. Of the two animals left in the successful group, for one

denying soma influence, the results in this case might as well lead to the conclusion that the offspring were from ovarian tissue of the mother, as from the engrafted ovarian tissue. Also in the remaining animal, from the description given of the post-mortem findings it is impossible to conclude that the mother's ovarian tissue was completely removed on both sides. This objection the authors endeavor to surmount by stating that the mass of ovarian tissue found at the site from which the right ovary was removed, was apparently strongly encapsulated, so that no ovum could be discharged even if it came to maturity. Such a conclusion is of course incompatible with the evidence, for few experienced pathologists, from the evidence presented, would care to make such a definite statement as to the retention of liberated ova.

Similarly, their statements regarding the regeneration of ovarian tissue are too absolute. For example, in certain cases where both ovaries were removed and ovaries from another animal engrafted in the neighborhood, as to the horn of the uterus, the absence of ovarian tissue at the site of implantation, and the presence of ovarian tissue at the site of removal of the animal's own ovaries is not proof that the former degenerated, and the latter regenerated. For it is possible that the implanted ovaries might have come in contact with the raw surface left after removal of the original ovaries, and become attached thereto. And since the engrafted ovaries were secured in place by means of exceedingly fine strands of unraveled silk, it is by no means certain that they could not have broken away from their moorings, owing to a cutting out of the tissues or a slipping of the knots, or even a breaking of the thread; though the latter accident would probably be less liable to occur.

These are merely some points that it is unsafe to leave out of account in concluding that such experiments are critical in the absolute sense, and I wish to say that I do not urge them as invalidating their results. In fact I consider that they have added at least one more confirmatory observation upon the reproductive functioning of transplantated ovaries, probably two, and possibly five. For the evidence does not absolutely rule out the animals which they have placed in the group in which they think regeneration of the ovarian tissue occurred. But it should not be forgotten that conclusions based upon indirect evidence, though appearing absolute, are never wholly free from at least a shadow of doubt. To accept this statement, it is only necessary to trace almost any biological subject developed from indirect experimentation a little way back into the literature. Indeed, teachings based upon such conclusions have passed without question through generations, to be later overthrown. And since the element of indirectness has not been eliminated in the experimental investigation of ovarian transplantation, I have stated that my results *seem* to lead to certain conclusions. And the same applies to Castle and Phillips's results as regards functioning of engrafted ovaries.

As to their interpretation of the results from the Mendelian standpoint, the nature of some objections to their conclusions has been discussed above. In addition, I would say that it is unfortunate that they did not preserve the individuals furnishing an ovary for engrafting, leaving the other ovary in place and then breeding this female to the same male used upon the female carrying the engrafted ovary. From their paper it would seem that they look chiefly to the second generation for evidence of soma influence, the index for detecting such influence being based upon the assumption that such influence would show in the second generation. The fallacy of this assumption has also been considered above.

In conclusion, I desire to say that the continuation and extension of these experiments is of the greatest interest and importance, and I hope that Professor Castle and his pupil may see their way clear to continuing them on a larger scale, using purer varieties of animals, including fowls of not too distantly related varieties.

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SCIENTIFIC BOOKS

Electric Motors. By H. M. HOBART. London, Whitaker & Co. 1910.

The name of Hobart is so well known to designing engineers in the electrical engineering field that any work from his pen needs no introduction to members of that profession. The volume under consideration being the second edition of his "Electric Motors" of 1904 is not altogether new to the electrical engineering fraternity, but there has been a considerable revision and modernization of the subject and an increase in the size of the book. The large amount of data included has also been revised to correspond to recent practise.

The scope of the book includes the electrical design, predetermination of characteristics, testing, operation and methods of special application of practically all types of motors now in commercial use, including the interpole D.C. motor and the single-phase series and repulsion A.C. motors.

Mr. Hobart's method of treatment is largely empirical and practical and each procedure is premised by copious data taken from tests.

It assumes a knowledge of the fundamental principles and familiarity with design. Thus it will serve better as a reference book for engineers and instructors than as a text-book for the student in technical schools.

No general method is given for the preliminary selection of dimensions for a design, but the reader is left to consult the data given. This is a common custom among designing engineers who, from their long experience and memory, can guess at the right proportions the first time, whereas the general practitioner or student needs some criterion for his selection. Few books on design lead up to the preliminary choice of design and dimensions by a discussion of those fundamental conditions which bear upon these proportions; such as allowable peripheral speed, allowable ampere conductors per inch or ampere stream on the armature and reasonable magnetic densities.

The book contains a very valuable compilation of data on various designs, the product of several different European manufacturers. This will make it a useful reference book for the American engineer, as it gives him information on the methods which others, having a different view-point, have used to solve those problems which he has met and probably solved in his own way. But it will be regretted by the general practitioner and the novice in designing, that American practise is not described. It is needless to say that designs which are very successful abroad could not be marketed here owing to the different conditions, particularly with reference to the cost of labor.

The treatment of the interpole motor, while very good, is not as extensive as a good many engineers would desire. There is need of a carefully systematized work of authority on this subject in English, something comparable to Arnold's work in German to which most designing engineers in direct-current as well as alternating-current work are obliged to refer more or less frequently.

The chapters on the Design of Induction Motors are very complete, and by means of the data included would enable one already familiar with the principles to produce very satisfactory designs; but there are certain features lacking and certain methods of treatment which would make it difficult for a person not in the habit of practising design to understand the subject. Thus the subject of the leakage reactance or inductance is treated almost altogether as a function of the "circle diagram" and no statement or diagram is given showing the paths of the leakage flux and the effect on the value of this leakage flux of the number and shape of the teeth.

While it is possible to take into account this leakage reactance in the calculation of the characteristics of the machine by means of the circle diagram by referring to data given, it is nevertheless very valuable to thoroughly appreciate during the actual process of design, exactly how much each particular feature of the design contributes to this quantity. This can only be determined by actually deriving the leakage path from the shape of the punchings. Incidentally, the effect of fractional pitch on the leakage reactance is

not very clearly brought out. Yet fractional pitch is very generally used in motors of American manufacture.

The chapters on the Design of Small Motors for Manufacture in Large Quantities and on Cost and Weight Coefficients are of undoubted value in concentrating attention on the factors which govern the expense, although the actual values being based on foreign practise would not be of great value to an American engineer.

In a book on design as comprehensive as this it seems a pity that some space is not devoted to the mechanical design. It is to be regretted that designers of the electrical features of apparatus are so dependent on the mechanical engineer to put their designs and ideas into execution.

Considerable space is given to the single-phase motor both of the induction, series and repulsion types, with the addition of very good introduction stating the logical limitations of the single-phase system.

The author and publishers should be congratulated on the excellent work shown in the cuts and curves which contribute considerably to the value of the data included in the book. This is really very extensive and alone would make the book of great value to the designing engineer as a book of reference.

WALTER I. SLICHTER

Testing of Electromagnetic Machinery. By B. V. SWENSON and B. FRANKENFIELD. New York, The Macmillan Co. 1911.

This volume is devoted to the testing of alternating-current machinery and is a sequel to the book on "Direct Current Machinery," previously published by the same authors. The book contains a description of a very large number of practical experiments illustrating the phenomena of alternating-current circuits and methods of testing commercial apparatus. It is intended to be used in technical schools in connection with a laboratory course.

The general scheme and methods are based upon the work which has been carried on in the laboratory of the University of Wisconsin under the authors, and contains additions and revisions due to the experience of Professor Bryant at the University of Illinois.

As a result of this collaboration and experience the text covers the field very completely and the methods advocated are those that would be generally conceded as the best and most practical.

The book is quite up to date both in its methods and in its scope, thus a treatment of the mercury arc rectifier and the split-pole converter are included, although the treatment of the latter is very brief.

It may be suggested that the experiments are resolved into too elementary and simple divisions and that a more efficient use of the student's time would be obtained by combining several of the experiments into one operation. There are 127 experiments listed, very few of which could be omitted from a good course, but these 127 could be logically grouped to cover the same ground in fewer operations.

For the theoretical basis and explanation of each experiment, the student is referred to a very large number of references in each experiment. The number of these references will in itself tend to discourage the average student to give any of them proper attention. It would be of more benefit to the student if a simple and concise development of the theory were included in the text with each experiment. However, for instructors in charge of courses these references so systematically arranged will be of great use.

WALTER I. SLICHTER

Economic Geology, with Special Reference to the United States. By HEINRICH RIES, Ph.D. Third edition. New York, The Macmillan Co. 1910. Pp. xxxiv + 589, pls. LVI., figs. 237. $3.50.

The importance of geology in its relations with mineral resources was recognized nearly a century ago in the establishment of official surveys. Still earlier in the European schools of mines the formation and classification of ore deposits were discussed in formal courses of lectures. But the growing development of agriculture, quarrying and mining has brought the science of geology more and more into the

foreground among subjects of importance in general education, and more and more courses in the purely scientific statement of the subject are followed up by those in its applications.

The text-book before us covers the latter field. It aims to carry a student through the various useful minerals and rocks; to instruct him in their modes of occurrence, the principles governing their accumulation and the statistics of their production. The non-metallics are first discussed, beginning with coal. Then follow in order, in Part I., petroleum and related hydrocarbons; structural materials; salines; fertilizers; abrasives; various minor minerals, and finally underground water. The author has freely used maps and pictures and summarizes literature at the close of each chapter. In the matter of clays and their applications he is especially at home from long experience with this particular line of investigation.

Part II. is devoted to the metalliferous deposits. An introductory chapter on the geological principles involved and the scheme of classification to be employed leads up to a systematic description of the ores of iron, copper, lead, zinc, gold, silver and the lesser metals. Again maps are freely used and with geological sections and pictures convey excellent ideas of occurrence and distribution. Statistics add the proper sense of perspective and of relative magnitudes.

The author writes with obvious knowledge and command of his subject. Successive years of presentation to classes and the two previous editions of the work have aided in bringing it to a high grade of excellence. The publishers have cooperated with maps and illustrations, with the result that a concise and very useful manual has resulted.

J. F. Kemp

PSYCHOLOGY IN RUSSIA

At the eighth annual meeting of experimental psychologists, held at Cornell University, April 17-19, 1911, Professor G. Tschelpanow, of the University of Moscow, described the status of psychology in Russia at the present time. He has been commissioned by his government to study psychological laboratories abroad, in order to perfect plans for the erection and equipment of a psychological laboratory building, the first and most complete of its kind—and to be established at Moscow, in the heart of Russia! This laboratory is the gift of Mr. S. I. Shtchukin, of that city, who has contributed 100,000 Rubel ($50,000) for the building and 20,000 Rubel for its equipment. He is already well known as a benefactor and protector of the modern school of painters, and has a large private museum of modern pictures which is often visited by English and French artists. The new laboratory is also endowed with a library of 3,000 volumes, worth 10,000 Rubel, presented as a memorial by the family of a young instructor of the University of Moscow, who met with an untimely death.

Professor Tschelpanow addressed the audience in German, but he kindly permitted me to translate the notes I had taken and to publish them, in spite of their sketchy, unfinished form, as I considered his remarks of general interest to scientists at large. He said in part:

"Experimental psychology in Russia is still in its beginning; although the first interest for it was aroused as much as twenty years ago. Its progress has been impeded partly by the uncertainty of political conditions, partly by the close affiliation of psychology with philology only, and not with natural sciences, and partly also by the fact that Russian universities have only collegiate rank, so that most of their advanced students still have to go to Germany for their research work.

"Among the older psychological laboratories, that at Odessa has become most widely known through the work of N. Lange. For some time he had but scanty space and only a few pieces of demonstrational apparatus at his disposal. At Kiew the laboratory consists of two rooms which contain demonstrational and other instruments. Moscow is in this respect the most fortunate place of all, because four years ago its laboratory was

started with four rooms and an initial endowment of 3,000 Rubel. It now has thirty students in experimental psychology, some of whom are undertaking independent work. Among the problems already attacked are: the study of reaction-types, Reuther's method of recognition, work on memory-types with the Binet method improved by controlled time-exposure, and the correlation of the three psycho-physical methods in regard to estimation of spatial extent. In the teaching of experimental psychology the Russian professors depend very largely upon translations of American text-books, especially those of Sanford and Titchener.

"In recent years applied psychology has become very popular and influential, through the work of Netschajeff and his cooperators, who have established about fifty psychological institutes at various *gymnasiums* and secondary schools where psychological instruction has been introduced. The method of making mental diagnosis has, however, reached a crucial point in Russia, inasmuch as strong opposition has set in toward a newly developed practise of outlining and analyzing mental abnormalities by reference to characteristic curves, especially when employed by comparatively inexperienced teachers. Objective or physiological psychology is represented chiefly by the well-known work of Bechterew and Pawlow, while interest in theoretical psychology still predominates."

After the meeting, Professor Tschelpanow showed and explained the architect's plans for the new laboratory, which is to be a three-story building. The basement will contain the heating plant, a workshop, a sound-proof room, space for animal psychology, large electric motors, and the apartments of the janitor and the mechanician. On the first floor, an auditorium with a seating capacity of three to four hundred persons and a room for demonstrational apparatus are provided for; furthermore, the director's office, the library, a room for collections of mental products, and a general writing room, are to be located here. The plan of the second floor makes allowance for a small lecture-room, for offices of the assistants, and for about twelve rooms in which the introductory courses for qualitative and quantitative experiments will be conducted. The third floor, finally, is to be given up entirely to research, and for this purpose it will be divided into twenty smaller rooms. A special feature on this floor is a large switchboard for the distribution of electric power. The building will be situated on university grounds, surrounded on all but one side by other university buildings, but removed as far as possible from public traffic. From all indications it promises to be an ideal home for the pursuit of psychological investigations, and it is to be hoped that the generous gift of Mr. Shtchukin will prove a fruitful example to other countries.

L. R. GEISSLER
CORNELL UNIVERSITY

THE TIME GIVEN BY UNIVERSITY STUDENTS TO STUDY AND RECITATION

IN connection with some committee work in Indiana University the writer was appointed chairman of a sub-committee to ascertain the time given by the students to their work.

It is thought that a brief summary of the results might be of general interest. Blank cards were handed to the students of all classes on Monday, February 14, 1910. The students were instructed to fill in the cards for all their courses. Each student was to fill out one card only, that is, if the student had an eight o'clock recitation, say, he filled out the card for all his courses. If he then went to another class, nine o'clock, say, he returned his card blank.

The cards called for the department, the number of course, the number of hours credit, the number of hours spent per week by the student in recitation or laboratory, and the number of hours spent per week by the student in home or library study. The card had blank spaces so that as many as seven courses could be filled in, if necessary. The total time spent by the students per week on a course was added and then divided by the number of credit hours, thus giving the time spent by the

individual student per week per credit hour for each course.

The cards were then arranged in alphabetic order. From the mid-term reports rolls of all the classes in the university were procured. By referring to the cards the time given by the student to the particular course was marked opposite the student's name, and the average time per week per credit hour was determined for each course.

By summing the totals of the courses and dividing by the total number reporting the average time was determined per instructor, and per department, as well as the grand average for the university.

Due to various causes, such as absence of students or forgetfulness of the instructors, reports were not obtained from all students. Approximately 75 per cent. of the students reported. About nine hundred cards were returned to the committee. A few of these were thrown out because they were not filled out properly. The cards showed that for a total of 4,438 registrations, 13,951.7 hours per week spent, or an average of 3.14 hours per week per credit hours (15 credit hours is regular work at Indiana University). That is, the average student spends 3.14 hours on each recitation. If the course is one in which no laboratory is required, he spends 1 hour in the class and 2.14 hours in preparation. If the course is a laboratory course requiring two-hour, two-and-a-half-hour, or three-hour periods, the student spends 1.14 hour, 0.64 hour or 0.14 hour respectively in outside preparation.

TABLE I

By Departments

Department	Enrollment	No. Reported	Hours per Week per Credit Hour
—	60	50	4.10 hours (highest).
—	304	255	3.81
—	5	4	3.74
—	244	138	3.53
—	956	658	3.53
:	:	:	:
—	93	78	2.68
—	63	51	2.68
—	107	80	2.52
—	71	50	2.34 (lowest).

In order to give an idea of the range I have arranged three tables—Table I., by departments, Table II. by instructors and Table III. by courses giving the enrollment, the number of students reporting, and the number of hours per week per credit hour, starting at the highest and ending with the lowest.

TABLE II

By Instructors

Instructor	Enrollment	No. Reported	Hours per Week per Credit Hour
—	33	22	4.25 hours (highest)
—	109	95	4.01
—	54	50	3.97
:	:	:	:
—	41	31	2.38
—	69	51	2.17
—	30	19	2.14 (lowest)

TABLE III

By Courses

Course	Enrollment	No. Reported	Hours per Week per Credit Hour
—	11	9	5.38 hours (highest).
—	31	25	5.07
—	15	11	4.38
—	4	3	4.30
:	:	:	:
—	10	9	1.92
—	4	3	1.66
—	24	19	1.25 (lowest).

It is true that the figures do not represent the facts in all cases. To the lazy student who has knowingly slighted his work the temptation would be great to increase the time of study. On the other hand, the plodding student would tend to underestimate his time. In certain cases the student may have heard that the proper time for the course was about so much. Under those conditions the average student will consciously or unconsciously make his figures correspond to the standard. However, the averages of large numbers may be taken to be near the true value. In any case the figures are not without interest.

R. R. Ramsey

Indiana University,
Bloomington, Ind.,
December 21, 1911

SPECIAL ARTICLES

BIOLOGICAL CONCLUSIONS DRAWN FROM THE STUDY OF THE TITANOTHERES[1]

THE chief object of this communication is to point out a possible harmony between the "continuity" and "discontinuity" theories of the phenomena of development.

The titanotheres, as an extinct family of mammals extending from the summit of the Lower Eocene to the summit of Lower Oligocene times, offer an exceptional opportunity for the solution of the two chief modern questions of evolution: first, the mode of transformation of existing characters; second, the mode of origin of new characters. The material available is now the most complete of any extinct family of mammals, embracing several nearly continuous series which branch out into a large number of phyla, some of which may be carried through all the phases of transformation. The investigation has been carried on with the aid of Dr. W. K. Gregory, and is marked by the introduction of a very exact system of measurements, whereby the various kinds of transformation may be studied in numerical terms. This exhaustive research appears to have a significant bearing upon the diverse theories of transformation entertained by the two modern schools of thought, the zoological and botanical, and the paleontological.

From the time of Waagen in 1869, who introduced the term "mutation" for the *stages* of the continuous development of certain inconspicuous but genetically important characters in Ammonites, the idea of continuity has become the established law among paleontologists. Neumayr added the conception of "*Mutationsrichtung*," that is, of trend or direction of development. The researches of Hyatt and Beecher were directed rather to the phenomena of recapitulation than specifically to the phenomenon observed by Waagen. Their writings, however, bring volumes of testimony to the law of continuity of development. This law has been established among vertebrates as well as invertebrates. The nature of the evidence presented to the paleontologist is entirely different from that presented to the zoologist; as an observer the former is practically immortal, that is, his range of observation, where it is possible to assemble continuous series of organisms, extends over enormous periods of time in contrast to the fleeting and essentially mortal glimpses which the botanist and zoologist may obtain of the fundamental processes of transformation.

Over against the idea of continuity, of definite development, and of certain trends of evolution, there have been developed among the students of living animals and plants (Bateson, de Vries and others) the notion of discontinuity and of order or orderly development produced only by selection. To this discontinuity de Vries has unfortunately applied the same term "mutation" which was introduced into biological literature by Waagen with entirely different significance; the name *saltation* should be attached to the de Vries hypothesis; until this is done we must speak of "mutations of Waagen" and "mutations of de Vries." The discontinuity conception has been strengthened rather than weakened by the wonderful revelations of Mendelian heredity, including the doctrine of unit characters and of "alternate inheritance."

The only tenet which the "continuous" and "discontinuous" schools of thought hold in common, or rather have reached in common, is that germinal evolution is the chief phenomenon upon which our attention must be concentrated. In the present communication the phenomena resulting from somatic changes or ontogeny, from environment, and from selection may be left out of consideration, and we may direct our thought solely and exclusively upon germinal evolution as it is displayed in the origin of new characters and in the transformation of existing characters in the titanotheres.

In all the sequence of the titanotheres only four kinds of change are observed: (1) *Increase of size.* This happens to be an almost

[1] Abstract of paper presented at the meeting of the National Academy of Sciences, Washington, April 19, 1911. Based upon the author's monograph "The Titanotheres," in preparation for the U. S. Geological Survey.

universal principle in this family, although it is by no means universal among mammals nor even among the Herbivora. (2) *Loss of parts.* This plays a very small part in the series of titanotheres as compared, for example, with the horses, since the chief parts lost are one element in the carpus, the trapezium and certain upper and lower incisor teeth. (3) *Changes of proportion.* This comprehends one of the most important and significant parts of titanothere evolution. Such change it is proposed to designate as "allometric," and new parts originating in this way may be termed "allometrons." (4) Continuous definite or *adaptive origins* of new characters, which the writer has previously termed "rectigradations."

Of the above phenomena (1) increase of size and of (2) loss of parts may be left out of consideration, and attention may be directed upon the (3) *allometrons* and the (4) *rectigradations.* It is found at once that their mode of appearance or the laws governing them are definite.

First, as to *rectigradations,* as exemplified by new cusps upon the teeth or by newly arising horns upon the skull, we find them subject to four important principles: (1) Rectigradations appear under the law of ancestral hereditary control, that is, the same rectigradations arise independently at different times in the descendants of remote common ancestors. This law has already been enunciated at a previous meeting of the academy and constitutes one of the most important generalizations brought out by the study of the titanotheres. (2) Rectigradations are continuous, arising from infinitesimal and almost invisible beginnings and passing into a stage of usefulness; this principle was pointed out many years ago by the author and described as "definite variation." (3) Rectigradations from the time of their first appearance are subject to the allometric influence of surrounding parts; thus a horn arising as a rectigradation in a brachycephalic skull will assume from the beginning a rounded form; arising in a dolichocephalic skull it will assume an elongate or oval form. (4) It is probable, but has not yet been demonstrated, that rectigradations are subject to fluctuations, that is, are more or less strongly developed around an average mean.

Second, the *allometrons* or changes of proportion follow partly the same and partly different laws than those pursued by the rectigradations. The most fundamental difference is the following: allometrons arise independently of remote ancestral hereditary control, that is, from a mesaticephalic ancestor there may arise, on the one hand, a dolichocephalic, and, on the other hand, a brachycephalic descendant; when, however, a trend of development, a law which appears to be coincident with the "*Mutationsrichtung*" of Neumayr, is once established then a tendency toward brachycephaly or dolichocephaly, respectively, becomes increasingly manifest; in this sense allometrons resemble rectigradations. The second law is that allometrons are continuous. This is positively demonstrated in certain phyla, and apparently will be demonstrated in all the phyla as soon as a full series or sequence is obtained. Any other theory of change of proportion but continuity is untenable in the face of the hundreds of measurements which especially demonstrate progressive brachycephaly. Measurements demonstrating progressive dolichocephaly and cytocephaly, or the bending down of the facial upon the cranial region of the skull, are based on less complete series.

It has been found convenient to introduce a series of cephalic indices of the ratios between breadth and length similar to the cranial indices used in anthropology. Thus, breadth $\div$ length gives the cranial index of a titanothere, and the gradual transformation from mesaticephaly into brachycephaly or dolichocephaly may be expressed in exact numerical terms. Every bone of the skull enters into these remarkable transformations, and every single bone has its own individual percentage of increment. The evolution of every part is differential. Thus there is no general stretching of the skull in dolichocephaly, as if it were composed of india rubber; the elongation may be confined to cer-

tain regions. It has recently been found that the ancestral titanotheres are dolichocephalic, of the type known as proöpic-dolichocephaly because the chief elongation is in front of the orbital region. Their descendants are also dolichocephalic, but the type is opisthopic dolichocephaly, that is, the chief elongation is behind the orbital region.

Similar allometric indices are also found in the limbs. For example, the ratio of the length of the tibia to that of the femur is very significant and is constantly changing in adaptation to weight and to speed.

Considering the transformation of the titanotheres in comparison with that of the horses and many other lines of mammals, where successive series have been obtained, we observe again exactly similar phenomena. It *appears that the law of continuity, of orderly and in a sense of predetermined transformation can now be established beyond refutation.*

The question then arises whether these laws of "continuity" can be harmonized with the potent demonstration that certain new characters and certain new proportions arise as saltations or discontinuously. The hypothesis which is here advanced is that continuity is the normal mode of development under natural conditions, that there are certain definite trends or tendencies, that there is in continuous series a "*Mutationsrichtung*," that by this continuous development the greater number of so-called "unit characters" have arisen, that occasionally, however, new unit characters may and do arise suddenly. The hypothesis may be expressed as follows: that the normal development of unit characters is a continuous progress, that under certain abnormal conditions, as of sudden change of environment, certain new unit characters may appear suddenly, that the cross-breeding of pure natural races in which unit characters have been built up by continuous processes breaks up these unit characters into a mosaic and gives rise to the larger part of the apparently saltatory or discontinuous phenomena which are being observed by the modern experimentalists.

As illustrations of this hypothesis, take as a very simple one the transformation of the head form in various human races; the development of dolichocephaly and of brachycephaly has in all probability been by continuous transformation in one direction or the other. In support of continuity is the evidence adduced among the titanotheres. When dolichocephalic and brachycephalic races intermingle, the fact that dolichocephaly or brachycephaly is a unit character appears at once in the non-blending of head form subject to the law of alternate inheritance. Another illustration is afforded by the results of the interbreeding of pure stocks of the horse, namely, according to the observations of Ewart, the Arab, or plateau type, the Przewalsky, or steppe type, and the draught, or forest type. Each of these pure original stocks apparently acquired by gradual transformation a very large number of distinctive characters displayed in the head, in the teeth, in the backbone, in the limbs, and last but not least in the psychic activities of these three great strains which have been bred for ages among very diverse environmental conditions. As soon as these three pure stocks are intermingled the fact that each is a mosaic of an enormous number of single, or unit characters becomes apparent in the mosaic type of horse which is produced, a horse showing singly or in groups various unit characters of the plateau, steppe or forest types. The transformation which, for example, has built up respectively the slender cannon bones of the desert and heavy cannon bones of the forest type has been, we have every reason to believe, a continuous, or progressive, or allometric change. On interbreeding, these slender or massive proportions may partly blend or may be detached as "units" from the progressively slender or massive head types to which they belong.

By far the greater number of the experiments carried on in support of the theory of discontinuity have been among hybrids, crossed strains, artificial strains, or strains subjected to unnatural changes of environment. It is important, therefore, for experimentalists to extend their work among abso-

lutely pure, natural races. Wherever nature is experimenting, as discerned by the field zoologist in the observation of geographic series from east to west, and north to south, from humid into arid regions, we are repeatedly finding geographically continuous series which shade into each other in color, in skull proportion, and limb proportion, and all other characters by continuous degrees of change.

Henry Fairfield Osborn

UNDERGROUND TEMPERATURES

It is an established fact that as the earth is penetrated below the limit of seasonal changes the temperature is invariably found to rise. Observations made in deep borings, wells, tunnels and mines have been sufficiently numerous over the earth's surface to indicate that the rise of temperature with depth "can not be explained on mere local causes." The rate of temperature increase is not uniform but is found to be quite variable, not only in different localities, but frequently in the same boring. This variation of heat increment is doubtless due to a number of causes,[1] such as differences in the thermal conductivity of rocks which vary in lithologic character, structure and contained water; inequalities of topography; circulation of water; chemical action; compression, etc. Whether the heat increment observed in the superficial zone continues to the center of the earth is not known, as observations are limited to only a little more than 1/4,000 of the earth's radius. Some investigators regard it as more probable that the rise of temperature diminishes below the superficial zone.

The conducting power of rocks was first accurately measured by Forbes,[2] and later by others. Forbes found that trap rock was the poorest conductor and solid sandstone the best. Sir Archibald Geikie[3] says, "the lighter and more porous rocks offer the greatest resistance to the passage of heat, while the more dense and crystalline offer the least resistance." The British Association Committee on Thermal Conductivities of Rocks[4] expressed the resistance of quartz by the number 114, basalt by 273, and cannel coal by 1,538. The same authority[5] records that heat travels four times as fast in foliated rocks, such as slate and schist, in the direction of cleavage than across it. It has been shown also that thermal resistance is lowered by the presence of interstitial water.

The subject of underground temperature attracted attention as early as nearly two centuries ago, when observations were made in the mines of Alsace by Gensanne in 1740, who found an increase of 1° F. in 50 feet. Among some of the earlier observers may be mentioned Saussure, Humboldt, Daubuisson, de Tebra, Forbes and Fox, Henwood, Cordier, De la Rive and Marcet, Phillips and others.

From 1868 onwards the British Association Reports contain valuable contributions by the committee on underground temperatures, and a summary is published in the volume for the year 1882. In 1886 Professor Prestwich's[6] valuable contribution on the subject of underground temperatures appeared, in which he collated all available data up to that time. This paper was later revised and published in his "Collected Papers on some Controverted Questions of Geology," 1895, pp. 166–279. Observations have been made and data bearing on this subject have been contributed at intervals to the literature from 1886 to the present time, with the conclusion that while there is an undoubted increase in temperature downward, the rate is more variable than was at first supposed.

Professor Prestwich gave the number of different localities and mines where observations were recorded as 248, and the number of stations 530. He found, with but few excep-

[1] Chamberlin and Salisbury, "Geology," Vol. I., 1904, pp. 544–547; Geikie, A., "Text-book of Geology," Vol. I., 4th ed., 1903, pp. 63–64.

[2] *Trans. Roy. Soc. of Edinburgh*, Vol. XVI., p. 211.

[3] "Text-book of Geology," 4th ed., 1903, Vol. I., p. 63.

[4] Rept. Brit. Asso. Adv. of Science, 1875, p. 59.

[5] *Ibid.*, p. 61.

[6] *Proc. Roy. Soc. of London*, 1886, Vol. XLI., pp. 1–116.

tions, that the earlier observations were inferior and of little value for purposes of accuracy, due chiefly, he says, to the imperfection of instruments and methods of experimentation. Because of the careful digest by Professor Prestwich of the voluminous data bearing on this subject, I regard it of sufficient interest to note briefly the following extracts from this valuable paper. The author classified the recorded results on underground temperatures into—(1) metallic mines, (2) coal mines, (3) wells and wet borings and (4) tunnels. The increase of temperature was found to be: (1) metallic mines, from 1° F. in 47 feet to 1° F. in 126 feet; (2) coal mines, from 1° F. in 45 feet to 1° F. in 79 feet; (3) wells and borings, from 1° F. in 41 feet to 1° F. in 130 feet; (4) tunnels—Mont Cenis, 1° F. for 79 feet; St. Gothard, 1° F. for 84 feet. The mean of these results gave 1° F. in 64 feet. Subsequent corrected readings in the two tunnels reduced the mean to 1° F. in 60 feet.

Professor Prestwich regarded the differences in results obtained in mines, wells, etc., indicated by an examination of the tables, to be attributed to the fact that the geological conditions were unlike, and the disturbing causes of a different order. The main disturbing causes in the different groups of openings made are stated and discussed. In coal mines they are stated as (1) loss of heat through exposed surfaces, (2) effects of ventilation, (3) other causes, such as crushing of rock, escape of gas, and effects of irregularities of surface (pp. 9–21). There are local variations according to structure, percolation of water, etc. From the reliable cases, the mean increase for coal mines was found to be 1° F. in 50 feet.

In metalliferous mines the main causes affecting thermal conditions are given as (1) ventilation, (2) percolation of water, (3) hot springs, due (*a*) to chemical decomposition and (*b*) to water coming from great depths[7] (pp. 25–34). The mean thermometric gradient was found to be 1° F. in 44 feet in rock, and 1° F. in 42.4 feet in springs, with an average for the two of 1° F. in 43.2 feet.

In wells and borings the main disturbing causes are regarded as (1) pressure on the instruments, and (2) convection currents. The mean thermometric gradient in this group of openings was found to be for non-flowing wells 1° F. in 51.2[8] feet, in flowing wells 1° F. in 49.1[8] feet, with an average for the two of 1° F. in 50[8] feet.

A paper entitled "Rock Temperatures on the Rand and Elsewhere," by E. M. Weston,[9] published in a recent number of the *South African Mining Journal* is of interest. The tables which follow below are taken from this paper.

Rock Temperatures in Depth on Witwatersrand Mines

Rock Temperature at	
1,000 feet	68.75° F.
2,000 feet	73.53
3,000 feet	78.35
4,000 feet	83.15
5,000 feet	87.95
6,000 feet	92.75
7,000 feet	97.55
8,000 feet	102.35

General rate of increase, 1° F. for 250 feet.

In the Lake Superior copper district, the Tamarack shaft is reported to have reached a depth of 6,070 feet, and the Red Jacket shaft a depth of 5,315 feet. The rate of increase in temperature is given as 1° F. for 209 feet. In a note published by Professor Alexander Agassiz in 1895, the greatest depth reached in the Calumet shaft was 4,712 feet, which showed an average increase in temperature of 1° F. for 223.7 feet.[10] Two bore holes are reported put down in Silesia to depths of 6,500

[7] In his revised paper, Professor Prestwich adds "the working operations" to the list of causes affecting thermal conditions in metalliferous mines. "Collected Papers on some Controverted Questions of Geology," 1895, p. 179.

[8] 52.2, 50.6 and 51.4 feet, respectively, in the revised paper. *Ibid.*, 1895, pp. 228 and 231.

[9] *South African Mining Journal*, November 12, 1910, p. 417.

[10] "On Underground Temperatures at Great Depths," *Am. Jour. Sci.*, 1895, Vol. L., pp. 503–504.

feet and 7,347 feet, with bottom temperatures of 158° F. and 181° F., respectively.

Rock Temperatures in Brazil Mines

(St. John Rel Rey Mine, Minas Geraes)

Rock Temperature at	
324 feet	70.0° F.
624 feet	71.0
924 feet	74.0
2,073 feet	78.0
2,824 feet	84.5[11]
3,724 feet	88.0
4,024 feet	95.0

Vertical depths of more than 4,500 feet are reported reached at Bendigo, Victoria, Australia, with a rock temperature of 110° F. at 4,000 feet. At the Adalbert mine in Bohemia the greatest depth is stated to be 3,600 feet, with a rock temperature between 3,500 and 3,600 feet of 113° F.

Rock Temperatures in Kalgoorlie Mines

Rock Temperature at	
1,400 feet	84° F.
1,700 feet	83
2,000 feet	83
2,300 feet	84

Data bearing on artesian-well temperatures in the Dakotas were tabulated and discussed by Darton[12] in 1898, which indicated very high and variable temperatures and for which no satisfactory explanation was offered. Records from 42 localities were given in which the depth of well varied from 432 feet to 2,500 feet, and the rate of temperature increase ranged from 1° F. in 17.5 feet to 1° F. in 45 feet, with an average of 1° F. in 35.4 feet. At the Pittsburgh meeting of the Geological Society of America, December, 1910, Mr. Darton read a paper entitled "A List of Underground Temperatures in the United States" in which he said: "The rate of temperature increase has been found to be very variable, but in places there is a marked relation to geologic features."[13]

[11] Equals sea level.

[12] Darton, N. H., "Geothermal Data from Deep Artesian Wells in the Dakotas," *Am. Jour. Sci.*, 1898, Vol. V. (N. S.), pp. 161–168.

The records of the Committee on Underground Temperatures, of the British Association for the Advancement of Science, show a range of 1° F. in less than 20 feet to 1° F. in 130 feet, with an average of 1° F. in 64 feet. Professor Prestwich concluded that the average increase in temperature was 1° F. in 47.5 feet (p. 55). Lord Kelvin assumed the rate of increase to be 1° F. in 51 feet. A lower rate of increase is indicated in more recent deep borings that have been carefully measured. From the data given above, quoted from the article by Weston on increase of temperature with depth in metalliferous mines, the general rate of increase in thermometric gradient for the different localities is: in the Witwatersrand mines from 1,000 feet to 8,000 feet, 1° F. for each 250 feet in depth; in the copper mines of the Lake Superior district, 1° F. for each 209 feet; in the St. John Rel Rey mine, Brazil, 1° F. for each 156 feet, approximately; and in the Kalgoorlie mines, Australia, practically no variation in temperature is indicated between the depths of 1,400 feet and 2,300 feet.

Professors Chamberlin and Salisbury give the following list of records:[14]

Locality	Depth in feet	Rise of 1°F.
Sperenberg bore (Germany)	3,492	in 51.5 feet
Schladeback bore (Germany)	5,630	67.1
Cremorne bore (N. S. Wales)	2,929	80
Paruschowitz bore (Upper Silesia)	6,408	62.2
Wheeling well (W. Va.)	4,462	74.1
St. Gothard tunnel (Italy-Switzerland)	5,578	82
Mt. Cenis tunnel (France-Italy)	5,280	79
Tamarack mine (N. Mich.)	4,450	100
Calumet and Hecla mine (N. Mich.)	4,939	103
Ditto, between 3,324 feet and	4,837	93.4

In commenting on these records the authors say:

[13] "Preliminary List of Papers, 23d Winter Meeting, Geol. Soc. America, Pittsburgh, Pa.," December, 1910, p. 2.

[14] "Geology," Vol. I., Geologic Processes and their Results, 1904, p. 543.

It is to be noted that even these selected records vary a hundred per cent. Very notable variations are found in the same mine or well, and often much difference is found in adjacent records, especially those of artesian wells. Some of these are explainable, but the full meaning of other variations is yet to be found.[15]

In conclusion, it may be stated that from recent figures bearing on this subject, no general law is observed in the increase of rock temperature with depth, and in general the increment of heat is lower and more variable than indicated by the earlier observers.

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THE SCALES OF THE DIPNOAN FISHES

I AM greatly indebted to Dr. G. A. Boulenger for scales of the few living members of the very interesting and remarkable subclass known as Dipnoi or Dipneusti. The result of their examination is quite surprising to me, and has, I think, an important bearing on their relationship with other fishes.

Neoceratodus forsteri, from Queensland, has very large oblong scales, one before me being 55 mm. long and 34 mm. broad, 20 mm. or less of the length being exposed in the living fish. In general appearance, the scales are not unlike those of *Heterotis,* except for size. The broad nuclear field, far apicad of the middle, is rugose; the circuli (fibrillæ) are all very fine, and both basal and apical are longitudinal; the basal fibrillæ are moniliform (minutely tuberculate), and in the lateral fields the whole surface is minutely rather irregularly tuberculate. Thus in its longitudinal fibrillæ *Neoceratodus* agrees with *Amia* and *Albula;* in having the fibrillæ tuberculate or beaded it agrees with *Albula* and the Osteoglossidæ. At first sight it seems that there are no radii in *Neoceratodus,* but closer inspection shows a complete system of fine radial reticulations, especially well developed in the lateral areas, where it accords perfectly with the network pattern of the Osteoglossids! This exceedingly characteristic feature is now known, therefore, in the Dipnoans, the Osteoglossids and the Mormyrids.[1]

Having determined these facts, I turned with eagerness to the material of *Lepidosiren* and *Protopterus.* In these fishes the scales are completely enclosed in the skin, but are, nevertheless, quite large (fully 8 mm. diameter in *Protopterus*), and shaped much as in *Osteoglossum.* Both have a strong radial network, while the circuli are reduced to innumerable fine tubercles or coarse granulations, approaching the condition of the lateral areas in *Neoceratodus. Protopterus annectens* from Africa (Gambia) and *Lepidosiren paradoxa* from Brazil have scales of entirely the same type, but in the *Protopterus* the network is more regular and more obviously similar to that of the Osteoglossids. In both the fibrillar granulations tend to run in lines near the margin, but this is rather more marked in *Lepidosiren;* the indications are in each case of longitudinal (not circular) fibrillæ. The general results may be thrown in the form of a table, thus:

(*A*) Basal fibrillæ longitudinal.
- (*a*) Fibrillæ moniliform or tuberculate.
  - (1) With radial network ... Dipneusti.
  - (2) Without radial network. *Albula* and *Dixonina.*
- (*b*) Fibrillæ not tuberculate; no radial network .... *Amia calva, A. scutata.*

(*B*) Basal fibrillæ circular (normal circuli); radial network present.
- (*a*) Fibrillæ tuberculate ..... Osteoglossidæ.
- (*b*) Fibrillæ not tuberculate ... Mormyridæ.

It is also to be remarked that *Gymnarchus* (Mormyridæ), *Heterotis* (Osteoglossidæ), *Lepidosiren* and *Protopterus* all have larvæ with external gills.

Dr. Boulenger has very kindly sent me the scales of the Osteoglossids *Scleropages formosus* from Borneo, *Scleropages leichardti* from Queensland and *Osteoglossum bicirrosum* from Cadajos, Brazil. They are practically circular (*S. leichardti* rather broader), and all have exactly the same structure, notwithstanding the wide geographical separation. The scales of *Heterotis niloticus* differ

[15] *Op. cit.*, pp. 543–544.

[1] For the last, see Smiths. Misc. Coll., Vol. 56, No. 3, p. 2.

in being oval. *Pantodon* (fam. Pantodontidæ) also has strongly tuberculate basal circuli.

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SOCIETIES AND ACADEMIES

THE NEW YORK ACADEMY OF SCIENCES

SECTION OF BIOLOGY

The regular monthly meeting of the Section of Biology held at the American Museum of Natural History, March 13, 1911, was devoted to a public lecture by Dr. George A. Soper, president of the Metropolitan Sewerage Commission, on the "Scientific Aspects of the Work of the Metropolitan Sewerage Commission."

In connection with the investigations of the commission upon the pollution of the waters of New York harbor from various sources, a great amount of scientific work has been done by Dr. Soper and his assistants. The most interesting features of this work and its results were dwelt upon in popular manner by the lecturer.

At the regular monthly meeting of the section held at the American Museum of Natural History, April 10, 1911, Chairman Frederic A. Lucas presiding, the following papers were read:

A New and Peculiar Porpoise from Japan: Roy C. Andrews.

The speaker exhibited photographs and parts of the skeleton of a new porpoise secured in the summer of 1910, in Rikuzen province, Japan. This specimen is allied to *Phocœna dalli* True, and with that species forms a distinct group of *Phocœna*-like porpoises which deserves generic rank. This group resembles *Phocœna* externally, but has white side and ventral areas sharply defined from the black of the upper parts, a falcate dorsal fin, and vertebræ numbering 95 or more. The type of the new genus to which *Phocœna dalli* was referred is the specimen which was secured in Japan, and has been formally described in a *Bulletin* of the American Museum of Natural History, now in press.

The Japanese porpoise presents characters, both externally and in the skeleton, which distinguish it from all other members of the entire family. The caudal peduncle shows a strongly marked "hump," and ventrally a prominent concavity which gives the posterior portion of the body a most extraordinary appearance. The neural spines of the entire vertebral series are extremely long and slender, reaching a height much greater than in any other known member of the *Delphinidæ*. The transverse processes are also very long and rod-like. The number of vertebræ is 95, approaching closely *P. dalli*, which has 97. The scapula is unlike that of any other member of the family in that its height almost equals its greatest breadth, and it is in general shape somewhat like that of a Baleen whale.

The specimen is, on the whole, one of the most remarkable members of the Delphinidæ that have thus far been discovered.

Observations on Birds and Fishes made on an Expedition to Florida Waters: J. T. Nichols.

This paper concerned itself with a trip through Florida waters on Mr. Alessandro Fabbri's yacht *Tekla* in the interests of the American Museum's department of fishes.

Attention was called to the abundance of the white ibis and Louisiana heron, contrasted with the scarcity of aigrette-bearing herons. After a brief mention of the work and the results obtained, the balance of fish-life in a fresh-water outlet of the everglades was compared with the balance of fish-life in the salt water as at Key West.

In the former situations gar pikes (*Lepisosteus*) were abundant, as were various Centrarchids (among them the large-mouthed bass and blue-gill sunfish) which darted in and out through the little channels among the weed, but which did not drive head first through the masses of weed as did the leathery-skinned gars, and only made quick sallies into the shallower and less open waters, where various species of Pœciliids, especially *Gambusia*, and *Fundulus goodei* were tremendously abundant. The surprising freedom from mosquitoes was mentioned and it was pointed out how the existing balance of fish-life was favorable to a great abundance of *Gambusia*, etc., which might be expected to prey on mosquito larvæ. The Centrarchids would be likely to hold in check a fish like the banded pickerel, which would have followed these small fishes into the shallows where the Centrarchids did not follow them, and perhaps materially reduced their numbers. The situation here where the large primitive gar, the spiny-rayed modern Centrarchids and the abundant intermediate Pœciliids made up the bulk of the fish population, was compared with the more complicated marine situation where large selachians and spiny-rayed basses, snappers, grunts, wrasses, scorpion fishes, etc., and schooling herrings and anchovies of various sorts in a way constituted homologous classes.

L. Hussakof,
Secretary